PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

FIRST MEETING,

HELD AT PHILADELPHIA, SEPTEMBER, 1848.

PHILADELPHIA:

PRINTED BY JOHN C. CLARK, 60 DOCK STREET.

1849.

COMMITTEE OF PUBLICATION.

ROBERT W. GIBBES, M.D.

B. SILLIMAN, JR., M.D.

J. B. ROGERS, M.D.

A. L. ELWYN, M.D.

W. S. VAUX, ESQ.

S. W. ROBERTS, ESQ.

The OFFICERS elected at the last meeting, held in Boston on the 24th September, 1847, were,—

WM. C. REDFIELD, Esq., *Chairman.*
Prof. WALTER R. JOHNSON, *Secretary.*
Prof. J. WYMAN, *Treasurer.*

STANDING COMMITTEE.

Prof. WM. B. ROGERS, } *Ex-officers,*
Prof. J. WYMAN, }
Prof. B. SILLIMAN, Jr. }
Prof. J. E. HOLBROOK,
Prof. H. D. ROGERS,
Prof. B. SILLIMAN,
Pres't E. HITCHCOCK,
Dr. S. G. MORTON,
Dr. CHARLES T. JACKSON,
J. D. DANA, Esq.
I. L. HAYS, Esq.

LOCAL COMMITTEE.

Dr. S. G. MORTON, *Chairman,*
Prof. ROBERT HARE,
Prof. S. S. HALDEMAN,
JAMES DUNDAS, Esq.
RICHARD C. TAYLOR, Esq.
Prof. J. B. ROGERS,
Prof. J. K. MITCHELL,
J. P. WETHERILL, Esq.
PETER A. BROWNE, Esq.

PROCEEDINGS,

&c.

First Day, September 20, 1848.

In conformity with a resolution of the "Association of American Geologists and Naturalists," adopted during its session at Boston, in September, 1847, that body agreed to resolve itself into the American Association for the Advancement of Science, and that the first meeting, under the new organization, should be held in the City of Philadelphia, on the third Wednesday (20th day) of September, 1848; and, agreeably to the arrangements and invitation of the Local Committee then appointed, the new Association held its first regular meeting this day, September 20, 1848, at the hour of 10, A. M., in the library room of the Academy of Natural Sciences of Philadelphia.

At 12, M., the meeting was called to order by Prof. Wm. B. Rogers, of Virginia, Chairman of the last Annual Meeting, who, after some preliminary remarks, read the draft of a Constitution and Rules of Order, which had been prepared by a committee, appointed for the purpose, at the meeting in Boston, in 1847.

The Chairman of the Local Committee then submitted letters, which had been received in reply to the letters of invitation issued by that committee, *accepting* the invitation, from Lieut. J. M. Gillies, of Washington; Samuel Henry Dickson, of New York; Z. Allen, Esq., of Providence, R. I.; Prof. J. C. Booth, of Philadelphia; Prof. J. S. Hubbard, of Washington, D. C., and J. H. C. Coffin, of Washington, D. C.

Letters, accompanied by promises to make communications to the

Association, were read by the Secretary, from Prof. Joseph Henry, of Washington, D. C.; Orson Kellogg, Esq., of New York; P. S. Hunt, Esq., of Norwich, Conn.; Prof. Stephen Alexander, of Princeton, N. J., and Prof. Chester Dewey, of Rochester, N. Y.

Letters, expressing the regrets of their authors at not being able to attend the present meeting, but manifesting, generally, a strong and abiding interest in the objects of the institution, were read from Prof. Edward Lasele, Williamstown, Mass.; Robert Gilmor, Esq., Baltimore; William Elliott, Esq., Henderson County, N. C.; George R. Perkins, Esq., Albany, N. Y.; Prof. Dennison Olmsted, New Haven, Conn.; Hon. Levi Woodbury, Portsmouth, N. H.; Richard C. Taylor, Esq., Philadelphia; Albert D. Wright, Esq., Brooklyn, N. Y.; H. C. Perkins, Esq., Newburyport, Mass., and Dr. J. B. S. Jackson, of Boston.

The Standing Committee, appointed at the last annual meeting, then offered the following resolution, which was, after some deliberation, postponed for the present, the draft of Constitution and Rules having previously been read from the chair.

Resolved, That the thanks of this Association be presented to the committee appointed at the Boston meeting, in 1847, to draft a Constitution and Rules; and that the draft prepared and circulated by said committee be adopted for the organization of the American Association for the Advancement of Science.

The Standing Committee also recommended that, for this day, the Association transact its business as a single body, in order to give time for receiving the titles of communications intended to be made to this meeting, and that the Standing Committee may be enabled to recommend such a division into sections, as the business before the Association may seem to require, which recommendation was agreed to.

The Secretary, Prof. Johnson, elected at the last annual meeting, then read the following communication:—

Hall of the Academy of Natural Sciences of Philadelphia,
September 20, 1848.

To the Secretary of the American
Association for the Advancement of Science.

Sir,—At a meeting of this Society, held last evening, the following resolution was unanimously adopted:—

Resolved, That the Secretary be instructed to address a letter to the officers of the American Association for the Advancement of Sci-

ence, inviting the members of the Association to visit the Hall of the Academy during their stay in Philadelphia.

Which I have the honour to transmit.

Very respectfully,

WM. GAMBEL, M. D.,

Recording Sec'ry.

Prof. JAMES B. ROGERS, on behalf of the Local Committee, presented the following letter:—

University of Pennsylvania, Sept. 5, 1848.

Dear Sir,—At a meeting of the Board of Trustees of the University of Pennsylvania, held this evening, I was instructed to tender the use of the college hall to the American Association of Geologists and Scientific Men, about to assemble in this city.

Will you be good enough to see the Provost upon the subject, and make such arrangements with him as will suit the Association and the college?

Very truly yours,

GEORGE EMLEN, JR.,

Sec. of the Board of Trustees.

DR. JAMES B. ROGERS.

Prof. ROGERS then stated that the above invitation had been accepted, and that accommodations for the meetings had been provided at the Hall of the University in Ninth street. WM. C. REDFIELD, Esq., offered a resolution, inviting the members of the Academy of Natural Sciences of Philadelphia to attend the sittings of the Convention, which was unanimously adopted.

PETER A. BROWNE, Esq., submitted a resolution, that the members, as a mark of respect for the memory of their late associate, Lardner Vanuxem, Esq., deceased since the last meeting, should wear crape on the left arm during the present session.

Dr. HARE would heartily concur in any suitable testimonials of respect to the excellent and estimable deceased member, but objected to the proposal of wearing crape, and suggested the possibility that before the close of the session the decease of other members might be brought to the notice of the Association. On his motion, seconded by Prof. J. B. Rogers and concurred in by Mr. Browne, the resolution was postponed for the present, and referred to the Standing Committee.

Prof. J. B. ROGERS stated that, in addition to the invitation from the

Trustees of the University of Pennsylvania, a similar invitation had been received from the Trustees of the Jefferson Medical College, to hold the meetings of the Association in their hall, when, on motion of PETER A. BROWNE, Esq., it was—

Resolved, That the thanks of this Association be tendered to the Trustees of the University of Pennsylvania, and of the Jefferson Medical College, for their kind offers of accommodation.

The Association then took a recess till 12 o'clock, in order to allow the Standing Committee time to prepare for a future division of the body into Sections, and to attend to other duties enjoined by the existing rules.

12 *o'clock, M., September* 20.

The first business, on re-assembling, was to have the reading of the draft of "Objects and Rules of the Association, prepared in obedience to a resolution passed last year at the meeting in Boston."

The draft was read, and its provisions explained, by the chairman.

The question of adopting the rules now submitted, for the permanent organization and government of the Association, was then put, and carried unanimously in the affirmative, as follows:—

OBJECTS AND RULES OF THE ASSOCIATION.

OBJECTS.

The Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction to scientific research in our country; and to procure for the labours of scientific men, increased facilities and a wider usefulness.

RULES.

Members.

RULE 1. Those persons, whose names have already been enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members on subscribing to these rules.

RULE 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members on subscribing to these rules.

RULE 3. The Collegiate Professors of Natural History, Physics, Chemistry, Mathematics and Political Economy, and of the Theoretical and applied Sciences generally; also Civil Engineers and Architects who have been employed in the construction or superintendence of public works, may become members on subscribing to these rules.

RULE 4. Persons not embraced in the above provisions, may become members of the Association, upon nomination by the Standing Committee, and by a majority of the members present.

Officers.

RULE 5. The Officers of the Association shall be a President, a Secretary, and a Treasurer, who shall be elected at each Annual Meeting, for the meeting of the ensuing year.

Meetings.

RULE 6. The Association shall meet annually, for one week or longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be entrusted to the Officers and the Local Committee.

Standing Committee.

RULE 7. There shall be a Standing Committee, to consist of the President, Secretary and Treasurer of the Association, the Officers of the preceding year, the Chairman and Secretaries of the Sections, after these shall have been organized, and six other members present, who shall have attended any of the previous meetings, to be elected by ballot.

RULE 8. The Committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this Committee to nominate the General Officers of the Association for the following year, and persons for admission to membership.

Sections.

RULE 9. The Standing Committee shall organize the Society into Sections, permitting the number and scope of these Sections to vary, in conformity to the wishes and the scientific business of the Association.

RULE 10. It shall be the duty of the Standing Committee, if at

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RULE 10. It shall be the duty of the Standing Committee, if at

any time two or more Sections, induced by a deficiency of scientific communications, or by other reasons, request to be united into one; or if at any time a single Section, overloaded with business, asks to be subdivided, to effect the change, and, generally, to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

Sectional Committees and Officers.

RULE 11. Each Section shall appoint its own Chairman and Secretary of the Meeting, and it shall likewise have a Standing Committee, of such size as the Section may prefer. The Secretaries of the Sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

RULE 12. It shall be the duty of the Standing Committee of each Section, assisted by the Chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communicasions are offered, and for the better forwarding the business, to assign the order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the Chairman to enforce these decisions of the Committee.

These Sectional Committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next Annual Meeting.

The Committees shall likewise recommend Reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent Annual Meetings.

Reports of Proceedings.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

Papers and Communications.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

General and Evening Meetings.

RULE 15. At least three evenings in the week shall be reserved for General Meetings of the Association, and the Standing Committee

shall appoint these and any other General Meetings which the objects and interests of the Association may call for.

These General Meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections, and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these General Meetings to receive the Address of the President of the last Annual Meeting, to hear such reports on scientific subjects as, from their general importance and interest, the Standing Committee shall select; also, to receive from the Chairmen of the Sections abstracts of the proceedings of their respective Sections, and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

Order of Proceedings in Organizing a Meeting.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting: the question of the most eligible distribution of the Society into Sections, shall then occupy the attention of the Association, when, a sufficient expression of opinion being procured, the meeting may adjourn, and the Standing Committee shall immediately proceed to divide the Association into Sections, and to allot to the Sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

Local Committee.

RULE 17. The Standing Committee shall appoint a Local Committee from among members residing at or near the place of meeting, for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements for the meeting.

Subscriptions.

RULE 18. The amount of the Annual Subscription of each member of the Association shall be one dollar, which shall entitle him to a copy of the proceedings of each meeting. The members attending an Annual Meeting shall pay, on registering their names, an additional assessment of —— dollars. These subscriptions to be received by the Treasurer or Secretary.

Accounts.

RULE 19. The Accounts of the Association shall be audited annually, by Auditors appointed at each meeting.

Alterations of the Constitution.

RULE 20. No Article of this Constitution shall be altered or amended without the concurrence of three-fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding Annual Meeting.

It was then, on motion,

Resolved, That the Association will meet this afternoon at 4 o'clock.

Wednesday, September 20, 4 P. M.

At this hour the Association again assembled, and Prof. W. B. ROGERS, who, as Chairman of the last Annual Meeting, had thus far presided at the organization of the present, now introduced his successor, WM. C. REDFIELD, Esq. President elect for the present year.

On taking the chair, Mr. Redfield expressed a diffidence in his ability to discharge satisfactorily the high duties which had been assigned to him, and on which he entered with unfeigned reluctance. He trusted, however, to the support and indulgence of the Association, for giving efficiency to his sincere desires and endeavours to fulfil the duties, which he only regretted had not fallen into abler hands. He distrusted his abilities to render so efficient aid to their deliberations, or to give so much satisfaction as the learned gentlemen who had preceded him in that office, but he would do his utmost in the station to which he had been called, and trust to the indulgence of his co-labourers for the result.

The President then declared the Association duly organized for the transaction of scientific business.

The first business before the Association, was the disposal of papers already entered on the docket, the reading to follow the order assigned by the Standing Committee.

The first paper read was by PETER A. BROWNE, LL.D., entitled—

SOME NOTICE OF THE FOSSIL CEPHALOPODES BELEMNOSEPIA, LONG KNOWN BY THE NAME OF "BELEMNITE," AND OF THE DIPHOSPHATE OF IRON, CALLED "MULLICITE," FOUND TOGETHER AT MULLICA HILL.

A mineral substance is found at Mullica Hill, Gloucester County, New Jersey, to which has been given the name of Mullicite. Dr. Thompson, in the first volume of his Mineralogy, has described this substance in an imperfect manner, owing no doubt to his not possessing sufficient specimens. Having in my cabinet, a number of them which exhibit the mineral in all its phases, I am induced to point out some of its peculiarities, and to endeavour to show its origin.

It will be recollected, that the fossil, long known by the name of "Belemnite," has been recently shown to be a portion of the skeleton of a Cephalopodes, which for convenience sake may be described as consisting of—1st. A circular wall of a chamber, in which the living animal preserved a sac containing an inky fluid, which it ejected to destroy the transparency of the water, to enable it to escape its enemies. 2d. A number of conical shaped pieces fitting into each other, forming as many chambers, all communicating by a central opening. 3d. A solid, straight, conical-shaped, fibrous portion, terminating in a point, and perforated throughout by a central tube or siphuncle.

Perfect specimens of this skeleton, found fossil in England, may be seen in the cabinet of the Academy of Natural Sciences of Philadelphia; and drawings and a description may be consulted in Buckland's Geology and Mineralogy. The only portions found at Mullica Hill, appertain to sections No. 2 and 3 of the above description.

Mullica Hill belongs to the tertiary formation, and consists of small grained gravel and sand, bound together by hydrated peroxide of iron, which abound in fossils in various stages of preservation; some of these (so far only as it is necessary to understand the Mullicite,) I will now attempt to describe; at the same time exhibiting the specimens.

Specimen No. 1.—This is a mass of the above noticed gravel and ferruginous sand, loosely aggregated, containing A—a portion of the petrified remains of this so called Belemnite; showing its straight conical shell (No. 3,) of the length of 2 inches, and of the diameter of 4-10ths of an inch; the anterior portion is broken off, but the accuminated posterior one is exposed; also the central tube an inch long,

and as thick as a line. The colour is brownish-black. B is a conical shaped cavity in the mass, 3 inches deep, and 3-10ths of an inch in diameter, in which one of these fragments has been. F is another cavity, with a diameter of 2-10ths of an inch. C is a circular opening, disclosing a fragment of the same portion of a Belemnite, 3-10ths of an inch across, and having a central tube 1-10th of an inch in diameter; the colour is black. Immediately alongside of E, is a compressed cavity, in which may be seen the central tube, 3-10ths of an inch long, and 1-20th in diameter; the colour white. In several parts of this mass may be seen the fossil remains of shells, but they are too much decayed to be recognised. The above remains of Belemnite fragments are clustered at various angles, but all having a general direction of the anterior extremities towards what was the surface of the conglomerate in situ.

Specimen No. 2.—This is a mass of yellowish-red ferruginous sand of the size of 2½ by 2 inches, enclosing the fossil remains of portions (No. 3) of five Belemnites. They are of a bluish-black colour, and of a vitreous lustre, and are about 3-10ths of an inch in diameter. M shows the accuminated posterior termination. C exhibits a small portion of the external surface of the skeleton. R has a longitudinal, white, central seam, but no central tube. All these are filled with a crystalline, mineral matter, of either a fibrous or lamellar structure, the fibres being stellated.

Specimens No. 3.—Here are three several fragments of the same portions of Belemnites, imbedded in the like sand. A is 2 inches long, and has a diameter of half an inch. The crystals are dark, bluish-black at one end, and ochreous-brown at the other. B is 1 inch and 3-10ths long, and 4-10ths of an inch in diameter, and is bluish-black throughout. C is 8-10ths long, and is fractured longitudinally, displaying the internal crystallization, and showing it to be lamellar; this fragment, therefore, probably belonged to portion No. 2, of the above division of the Cephalopodes.

Specimens No. 4.—Two isolated fossilized portions of No. 3 of Belemnites, both having the accuminated posterior terminations, but M having it most perfectly. O is encrusted with the ferruginous sand which has been removed from M. They are, respectively, about 2½ inches long, and half an inch in diameter. The colour outside, ochreous-brown; the crystallization is lamellar distinct concretions, of a bluish-black colour.

Specimens No. 5.—Two fossilized, isolated portions of Belemnites, that have fallen out of the sand; they are 2 inches and 8-10ths long,

and 4-10ths to 5-10ths in diameter. The colour, dark bluish-black. N shows the accuminated posterior termination of portion No. 3, and one of the chambers of portion No. 2, at the other extremity. The crystallization of portion No. 2 is lamellar, and that of No. 3 is fibrous; the fibres radiating from the centre of the fossil.

Specimens No. 6.—These are fragments of fossilized portions of part No. 3. The colour and crystallization resemble those of specimens No. 5. The termination of A shows the stellated structure to perfection. D are twin fossils.

Specimens No. 7.—Here are eight fragments of the same portions of the skeleton, in various states of preservation. A exhibits a small part of portion No. 2. All these specimens are referable to the Cephalopodes Belemnosephia, the fragments of portions No. 2 and 3 of which have been denominated "Belemnites." It is probable that the portion No. 1 was more fragile, as more of them are found at Mullica Hill.

We come now to speak of a different kind of fossil.

Specimens No. 8—Are two crystallized masses of a conchoidal shape; one of them 2 and 3-10ths by 1 and 1-10th inches, the other 2 and 4-10ths by 1 and 7-10ths inches. The general figure is concavo-convex, and the appearances indicate that they emanated from *bivalve shells.* The colour is bluish-black, the crystallization lamellar, fasciculated, the bundles radiating; or lamellar and lanceolate, the lances stellated.

Specimens No. 9.—Three similar to No. 8, only of smaller dimensions.

Specimens No. 10.—Six fragments where the glabrous texture of the former shell is preserved. Colours, brown, blue, and cinerous.

Specimen No. 11.—The cast of a fossil Grypha. Colour, brown.

Specimen No. 12.—The fossil remains of an Ostrea; same colour as the last.

Numerous casts and fossils, such as the two last, are found at Mullica Hill, which enhance the probability that Specimens 8, 9, and 10, emanated from *bivalves.*

Specimens No. 13.—Groups of crystals, the fasciculated and concentric structure imparting an alated appearance. Sometimes the wings are so fascicled as to make the mass reticulated.

Specimens No. 14.—More varieties of the same.

Specimens No. 15.—Adnate and single crystals, showing the lamellar, lanceolate and filiform structure, and metallic colour.

Upon an examination, the mineral was found to be phosphate of iron.

From a comparison of these specimens, it is apparent that the "congeries of small needles," described by Dr. Thompson as radiating from the centre of the fossilized Belemnite, are not the true crystal of the mineral substance (diphosphate of iron), as he supposed, but are due to the *former structure* of portion No. 3, of the Cephalopodes.

When crystallization takes place in any of these fossils, except the portion No. 3, of the Cephalopodes, these congeries do *not* make their appearance. But I admit, that from the apex of the fossil remains of the bivalve shells, there is a tendency in the bundles of lamellar and lanceolate crystals to radiate, as may be seen in Nos. 8, 9, and 10.

In cases of the fossil remains of portions No. 2, of the Cephalopodes, wherever the mineral substance has had *space sufficient to crystallize freely*, the *fibrous* figure is lost in the lamellar structure. See No. 3, C.

At the time that the animals, whose fossil remains are now there found, were living inhabitants of Mullica Hill, the iron was disseminated in the ferruginous sand. As these animals, after dissolution, surrendered their phosphoric acid, it combined with the iron and water, forming the *diphosphate of iron;* and as the operations of decomposition and transmission were *gradual*, it is natural that the new mineral should take the structure and form of the former animal substance.

Phillips, in his Mineralogy, p. 210, speaking of blue iron (phosphate of iron), says, that in Siberia it is found in fossil shells, but he does not describe its crystallization.

The second paper read, was—

On the Identity of the Atops Trilineatus and the Triarthrus Beckei (Green), with Remarks upon the Eliptocephalus Asaphoides. By Professor E. Emmons.

After the question of the identity of the two fossils had been reported upon by a committee of this Association, it may appear superfluous to bring up the subject again before this body; but inasmuch as a few individuals still maintain the opinion adverse to that expressed by the committee, and as other specimens, more perfect, have since come into my possession, I deem it proper to call the attention

of the Association once more to the question. In doing this, however, I shall not attempt to answer formally the objections to the report of the committee, but proceed directly to describe the two fossils indicated. The accompanying figures will aid me in conveying a correct idea of the strongly marked differences which they exhibit.

1. *Description of the Atops Trilineatus.*

CRUST-granulated; CEPHALIC shield semicircular, with the anterior and lateral edges turned upwards; posterior angles rounded; length and breadth of the shield as 1 to 3; glabella slightly narrowed, and turned upward anteriorly; divisions 4 or 5, and unequal; the 4th division is marked by a sulcus, which runs obliquely downward and backward, and meeting in the median line or axis; the posterior division runs parallel with the ribs, or rings of the body, and has one protuberance upon the median line. FACIAL SUTURE-EYES; cheek shields, rounded at the posterior angles. BODY composed of 17 or 18 rings, which become narrow toward the caudal extremity; axis narrower than the lateral lobes; rings 17, and which are separated by a groove about as wide as the ring itself, and marked by a row of spinous points upon their median line; lateral lobes or ribs, 18, jointed; the first, or anterior, corresponds with the divisions of the glabella; axis of the lateral lobes marked by a strong prominence, which becomes obsolete toward the tail; rings grooved by a single and simple sulcus, which runs directly outward, but which suffers a slight interruption at the axial prominence: the caudal shield is composed of one ring, and bordered by a flat expansion of the crusts.

2. *Description of the Triarthrus Beckei.*

Crust smooth, semicircular parabolic, bordered by a slightly thickened edge, traversed by a groove; posterior groove terminated in a point, which turns inward, and which extends beyond the base; height and breadth as 1 to 2. Glabella rectangular on three sides, and rounded only anteriorly; length and breadth equal; dimensions 4, and marked by three oblique lines, which run from the border downwards, the last only meeting in the median line: body composed of 12 or 13 rings, whose width is one-third greater than that of the lateral lobes; spinous-like points mark the median line or axis; lateral lobes composed of 12 or 13 rings; rings marked by an oblique furrow running from the upper and inner angle to the opposite lobe and outer angle, and by a short direction, which, if continued, would intersect the other near the middle of the rib or joint;

caudal shield composed of four or five rings, and bordered by a thickening of the crust, which are marked by fine lines.

Observations.

From the foregoing descriptions, it will be observed that the proportions of the two fossils are quite different. These differences begin with the cephalic shield, and extend to the tail; not only affecting the major parts, but extending also to the minor parts, in many minute details. The dissimilarity, in fact, is so great, that I am quite unwilling to regard them even as belonging to the same genus. In the paleontology of New York, parts of the same fossil, which I have called the *Atops Trilineatus*, are described as the Triarthrus Beckei, or rather as *Calymone Beckei*. I am not, therefore, in fault, as regards the specimens from which the foregoing description has been drawn.

Remarks upon the Eliptocephalus Asaphoides.

The *Eliptocephalus*, although acknowledged a new and different species from any that had been described previous to the publication of the Taconic system, has been referred to the genus Olenus. This view of the fossil, if sustained, does not affect the validity or stability of this system. Still, convinced as I am of the dissimilarity of the genera, I must avail myself of this opportunity to reassert my claim to the genus.

The *Olenus* has 14 joints, but the fragment of the body of the *Eliptocephalus* has 15 or 16 visible; and we may conclude, from appearances, that 4 or 5 are broken off; making, probably, in all, 20 joints. In this respect it is closely allied to *Paradoxides*. The *Olenus* has an axis only one-fourth the breadth of the body, while the axis or middle lobe of the eliptocephalus is nearly equal to the lateral lobes. The axis, again, of the *Olenus*, and of the *Paradoxides* also, is strong and prominent, while in the *Eliptocephalus* it is only slightly elevated, the distinction between the middle and lateral lobes is quite obscure and imperfect. Again, the cephalic shield is about twice as wide as high, in the *Eliptocephalus*, while in the *Olenus* it is three times as broad as high, and the glabella is only one-fourth as wide as the shield.

There are no divisions of the glabella of the *Eliptocephalus*, as indicated in the paleontology of New York, but on the shield there is a strong elliptical elevation, which furnishes the name to the fossil. So the margin is not reflexed as in Olenus. In conclusion, I may remark that the Eliptocephalus may belong to the family of the Oleni-

DAE, as proposed by Burmeister; but it still differs too much in its cephalic shield from both the Paradoxides and Olenus, to belong to either genus.

Thus far, and up to September, 1848, no fossil has been discovered in the Taconic rocks, which is common to the Taconic and New York systems.

The trilobite and fossils of the geologist of Vermont, (Mr. Adams,) which were found in the lower rocks about Lake Champlain, belong to an intercalated mass of shaly limestone, associated with calciferous sandstone, and is placed only in juxtaposition with the Taconic slates, but is never interlaminated with them, and really belongs to the New York system.

I make these remarks upon the fossils because they appear to me to be called for; but I wish it to be fully understood, that the separation of the Taconic rocks from the New York, or Silurian, is called for, and *was* called for, on grounds far more important than the presence of certain fossils; that evidence is found in the succession and superposition of the two systems, and in the vastness of the Taconic system, compared with the Silurian—the slates of the Taconic system being at least four times as thick as all the members of the Silurian put together.

The following communication, addressed to the Association, by Dr. JOHN W. VANCLEVE, of Dayton, Ohio, was then read by the Secretary, accompanied by the exhibition of six sheets of etched illustrations of the new species of zoophytes, described in the paper.

To the American Association for the Advancement of Science.

For several years past I have employed some portion of my time in preparing a work on the "Fossil Zoophytes of Western Ohio, with a few additions from other western localities," in which all the corals of this region, that have come under my observation, shall be figured and described. I have engraved the figures, chiefly, by etching them upon copper, for printing in letter-press; so that they shall come into the pages of the body of the work, in conjunction with the descriptions. The generic characters, and those of all the species not considered new, have been translated from the work of Goldfuss, the technical description from the Latin being followed by the familiar and more extended observations from the German, with the addition of the localities in this country, and occasional remarks. In describing species believed to be new, the technical description has been fol-

lowed, as in Goldfuss' work, by other remarks corresponding to those in German.

The fossils described having chiefly been found in the vicinity of Dayton, some notice of the various formations in which they occur may be proper. They consist of five successive calcareous deposits, which are exposed at various places within a distance of three miles from the town, all differing very much from each other, in general appearance, texture, colour, and in the fossils which they contain. The lowest, universally called the "Blue Limestone," corresponding with the Lower Silurian, consists of innumerable layers, varying in thickness from less than a fourth of an inch to eight inches or more, with layers of marl of similar varying thickness intervening. It is the only rock which appears at the surface in the south-western corner of Ohio, and the adjoining parts of Indiana and Kentucky. It is principally made up of bivalve shells, but contains numerous corals and fragments of trilobites and orthoceratites. The corals are of the genera Astrea, Anlopora, Ceriopora, Cyathophyllum and Retepora; and none of the species found in this formation have ever yet been found in those above it. The same circumstance is believed to be true with regard to the species of shells. The colour of the fossils is the same as that of the stone in which they are imbedded.

Upon the Blue Limestone lies a bed of marl, varying in its thickness, at different points, from ten to sixty feet. It does not include any solid rock, either in layers or fragments; being of the same composition, texture and colour, from top to bottom, although its colour varies a little at different places, at some distance from each other. No fossils have been observed in it.

On this bed of marl rests a formation of limestone, usually between twenty and forty feet in thickness, composed of the remains of a great variety of animals, intermixed with semi-crystalline scales. Sometimes, fragments of encrinites constitute almost half the mass. The colour is variable; in some places nearly white, in others reddish, in others dusky brown; but it is most frequently of a wax yellow, and the formation is usually termed the "Yellow Limestone." The fossils found in it, besides the encrinital remains, are chiefly corals, with a very few shells and trilobites. The corals are generally in excellent preservation, differing very little, if at all, in colour and texture, from recent specimens. Their colour is of a bony whiteness, not being influenced by that of the rock in which they occur, whether white, red, brown or yellow. They are of the genera Astrea, Catenipora, Cellepora, Cyathophyllum, Eschara, Favosites,

Flustra, Gorgonia, Lithodendron and Retepora. Of these genera, none of the species found in this formation are seen in either of those above or below it, except the Catenipora, Eschara and Gorgonia. The Catenipora is found in the Quarry Limestone, and its impressions, and those of the Gorgonia and two species of Eschara, occur in the Silicious Limestone.

Upon the Yellow Limestone rests a formation, which is usually termed the "Quarry Limestone," as, on account of its superior quality, it is almost the only stone quarried in the vicinity for building purposes. It consists of several layers, easily separated from each other, making an aggregate thickness of about four feet, which it is not known to exceed. It is of a light grey colour, and very hard and compact. The fossils which it contains differ but little in colour from the stone, and are so completely enveloped in it, that they are not easily found. The genera that have been observed are Astrea, Catenipora, Favosites, Sarcinula and Syringopora. The species are all peculiar to this formation, except those of the Catenipora, as mentioned before.

The Quarry Limestone is covered by a formation of "Silicious Limestone," in which the silicious matter, in fine grains, constitutes about fifteen per centum. The rock has the colour of ashes. It is six or seven feet thick, near Dayton; but as it extends to the north and east, the thickness increases to fifty or sixty feet. It usually contains only the impressions of fossils, the whole substance of the shell or coral having, in most cases, disappeared. The corals, of which impressions are left, belong to the genera Catenipora, Coscinopora, Eschara and Gorgonia.

To the last three formations, collectively, Dr. Locke, in the Ohio Geological Reports, has applied the general designation, "Cliff Limestone." They do not pass imperceptibly into each other, by a gradual alteration of colour, texture and general appearance; but the change is immediate and total, the lines of separation being definitely marked; and the fossil characteristics, with the exceptions already mentioned, are also entirely different.

The gravel, in the beds and banks of the streams, and in the neighbouring hills, affords a great variety of fossils. Although the principal part of it is derived from the limestone of the vicinity, a very considerable portion of it is made up of pebbles which have a different origin. These consist of fragments of primary rocks, of all the various kinds seen in the boulders scattered over the country, and of numerous fossiliferous varieties of calcareous and silicious rocks, the

fossils of which are generally very different from those found in the limestone of the neighbourhood. They belong to the genera Astrea, Aulopora, Catenipora, Columnaria, Conodictyum, Cyathophyllum, Eschara, Favosites, Gorgonia, Lithodendron, Stromatopora and Syringopora.

The following is a List of the Corals, the Plates and Descriptions of which are ready for the Press.

Agaricia swinderniana, Goldf., Louisville, Ky.
Astrea serradiata, Goldf., gravel, Dayton.
  „ porosa, Goldf., gravel, Dayton; Huntington, Ind.
  „ flexuosa, Goldf., gravel, Presq' Isle Bay, Lake Huron.
  „ collata, new species, blue limestone, Dayton.
  „ prismatica, new species, gravel, Dayton. (Calcareous.)
  „ vesiculosa, new species, quarry limestone, Dayton.
  „ septa, new species, quarry limestone, Dayton.
  „ venusta, new species, gravel, Däyton and Presq' Isle Bay.
  „ concentrica, new species, gravel, Dayton.
  „ basaltica, new species, gravel, Dayton.
  „ madreporacea, new species, Defiance, O.
  „ tubulata, new species, yellow limestone, Dayton.
Aulopora spicata, Goldf., gravel, Dayton.
  „ intermedia? Munster, blue limestone, Dayton.
  „ serpens, Goldf., Madison, Ind.
  „ nitida, new species, quarries, Sandusky, O.
  „ lata, new species, Louisville, Ky.
Catenipora escharoides, Lam., cliff limestone, Dayton.
  „ labyrinthica, Goldf., cliff limestone, Dayton.
Cellepora verrucosa, new species, yellow limestone, Dayton.
Ceriopora multiformis, new series, blue limestone, Dayton.
  „ tenuis, new species, blue limestone, Dayton.
  „ mammillata, new species, blue limestone, Dayton.
  „ muricata, new species, blue limestone, Dayton.
  „ constellata, new species, blue limestone, Dayton.
  „ tuberculata, new species, blue limestone, Dayton.
  „ ensiformis, new species, blue limestone, Dayton,
  „ orbiculata, new species, blue limestone, Dayton.
Columnaria alveolata, Goldf., Dayton; quarries, Madison, Ind.
Conodictyum torulosum, new species, gravel, Dayton.
Coscinopora macropora, Goldf., silicious limestone, Dayton.
  „ sulcata, Goldf., lead region, Iowa.

Cyathophyllum picatum, Goldf., blue limestone, Dayton.
„ seratites, Goldf., yellow limestone, Dayton.
„ excentricum, Goldf., Louisville, Ky.
„ flexuosum, Goldf., gravel, Dayton; Lake Huron.
„ vermiculare, Goldf., gravel, Dayton. (Silicious.)
„ turbinatum, Goldf., gravel, Dayton; Defiance, O.; Delaware Co., O.; Louisville, K.
„ hypocrateriforme, Goldf., gravel, Dayton. (Silicious.)
„ helianthoides, Goldf., yellow limestone, Dayton.
„ caespitosum, Goldf,, gravel, Dayton; limestone, Columbus, Delaware and Sandusky, O.; Madison, Ind.; Louisville, Ky.; Shawneetown, Ill.

Cyathophyllum dianthus, Goldf., gravel, Dayton.
„ ananas, Goldf., gravel, Dayton; limestone, Defiance, O.
„ hexagonum, Goldf., gravel, Dayton and Lake Huron; limestone, Sandusky, O., Louisville, Ky., Shawneetown, Ill., and Iowa.
„ visiculosum, Goldf., gravel, Dayton; limestone, Delaware Co., Ohio.
„ secundum, Goldf., gravel, Dayton.

Eschara puncta, new species, yellow limestone, Dayton; Louisville.
„ lobata, new species, yellow limestone, Dayton.
„ bipunctata, new species, yellow limestone, Dayton.
„ perforata, new species, yellow limestone, Dayton; Columbus.
„ multifida, new species, yellow limestone, Dayton.
„ ramosa, new species, yellow limestone, Dayton.
„ bifurcata, new species, yellow and silicious limestone, Dayton.
„ canaliculata, new species, gravel, Dayton; Sandusky, O.
„ compressa, new species, yellow limestone, Dayton.
„ dendroidea, new species, yellow and silicious limestone, Dayton.

Favosites alveolaris, Goldf., yellow limestone, Dayton.
„ favosa, Goldf., quarry limestone and gravel, Dayton; Drummond's Island, Lake Huron.
„ gothlandica, Goldf., gravel, Dayton; Drummond's Island.
„ basaltica, Goldf., gravel, Dayton; Lake Erie.
„ infundibulifera, Goldf., gravel, Dayton; Lake Huron.
„ polymorpha, Goldf., four varieties, gravel and yellow limestone, Dayton.
„ spongites, Goldf., two varieties, gravel and yellow limestone, Dayton; from Lakes to Natchez.
„ fibrosa, Goldf., two varieties, gravel and yellow limestone, Dayton.

Flustra lanceolata, Goldf., yellow limestone, Dayton.
Gorgonia infundibuliformis, Goldf., yellow and silicious limestone and gravel, Dayton; Defiance, Sandusky, Delaware and Columbus, O.; Louisville, Ky.; Bloomington, Ind.
Lithodendron dichotomum, Goldf., gravel, Dayton.
„ caespitosum, Goldf., yellow limestone, Dayton.
„ verticillatum, new species, gravel, Dayton.
„ vermiculare, new species, gravel, Dayton.
„ larviforme, new species, gravel, Dayton; limestone, Columbus.
„ secundum, new species, limestone, Columbus.
„ faściatum, new series, limestone, Delaware, O.
„ flexuosum, new species, gravel, Dayton.
Retepora prisca, Goldf., gravel, Dayton.
„ producta, new species, yellow limestone, Dayton.
„ terebrata, new series, gravel, Dayton.
„ contexta, new series, limestone, Louisville, Ky.
„ nitida, new series, blue limestone, Dayton.
Sarcinula organnum, Lamarck, quarry limestone, Dayton.
„ costata, Goldf., Iowa.
Syringopora verticillata, Goldf., gravel, Dayton; Drummond's Island, Lake Huron.
„ ramulosa, Goldf., gravel, Dayton.
„ reticulata, Goldf., quarry limestone and gravel, Dayton; Defiance, O.; Bloomington, Ind.
„ caespitosa, Goldf., Hardin Co., Ill.

I have specimens of stromatopora concentrica and polymorphia, and of some other species of the foregoing genera, chiefly new, amounting altogether to fifteen or twenty in number, of which the plates and descriptions have not been prepared. I could complete them at any time by the labour of a few weeks; but, as it is a matter of much doubt whether the work, if published, would pay expenses, and compensate me for what I have done, I have suffered a year to pass without adding any thing to my list of engravings. Whether I shall go on with them or not, will depend very much on the prospect of a satisfactory arrangement with regard to the publication.

I send with this, plates of all the new species in the foregoing list, and also a page printed as a specimen of the manner in which I designed to arrange the matter with the cuts in connexion with the descriptions.

JOHN W. VANCLEVE.

On motion of Mr. S. W. ROBERTS, it was—

Resolved, That the first General Evening Meeting of the Association shall be held on Friday evening next, the 22d instant, at half past seven o'clock, in the College Hall of the University of Pennsylvania; and that it shall be the duty of the Standing Committee to make arrangements for bringing before this and the subsequent evening meetings, one or more subjects of general interest, and—

Resolved, further, That each member of the Association shall be permitted to introduce one or more ladies to any of the meetings.

The President stated that, according to the Constitution, six members of the Standing Committee, to act in conjunction with those who are ex-officio members of that Committee, are to be elected at each meeting, by ballot, from among the members present, who have attended any of the previous meetings. It was then, on motion—

Resolved, to proceed to an election of six members of the Standing Committee, when, on canvassing the votes, it was found that the following gentlemen were duly elected members of that Committee, viz:—

Prof. BENJAMIN PEIRCE, Prof. S. S. HALDEMAN, Prof. JOSEPH HENRY, Prof. LOUIS AGASSIZ, RICHARD C. TAYLOR, Esq. and PETER A. BROWNE, Esq.

Prof. JOHNSON stated, that the great amount of business likely to come before the Association would evidently require some aid, such as had been formerly afforded to the Secretary in the fulfilment of his duties, and moved the appointment of two Assistant Secretaries; which motion was adopted, and, on nomination,

Prof. LEWIS J. GERMAIN and Dr. M. W. DICKESON were appointed Assistant Secretaries.

Mr. BROWNE then called up his resolution, offered this morning, in respect to Prof. LARDNER VANUXEM, deceased, which was referred to the Standing Committee.

On motion, the Association adjourned to meet to-morrow morning at 10 o'clock.

Attest, WALTER R. JOHNSON, *Sec'ry.*

Thursday, 10 *o'clock, A. M., Sept.* 21.

The Association again met, agreeably to adjournment, in the Hall of the University, WILLIAM C. REDFIELD, Esq., President, in the chair, a large number of additional members being present, and many

new subjects being announced for entry on the list of communications, intended to be presented during the Session.

A communication was read from the Corresponding Secretary of the Franklin Institute, conveying a resolution of the Board of Managers of that Institution, inviting the members of the Association to attend a Monthly Meeting of the Institute, to be held to-morrow evening. On motion, the invitation was accepted, and the thanks of the Association were tendered for the kindness manifested by the Managers of the Institute.

Prof. WILLIAM B. ROGERS, as Chairman of the Standing Committee, reported a programme for the organization of the Association, into Sections, agreeably to the requirements of the Constitution, and offered the following resolution, which was adopted:

Resolved, That the Association be divided into two Sections, one to embrace General Physics, Mathematics, Chemistry, Civil Engineering, and the applied Sciences generally; the other to include Natural History, Geology, Physiology and Medicine.

On motion, it was—

Resolved, That the order of proceedings at the General Meeting, to be held on Friday evening, be as follows:

1. Annual Address, by Prof. W. B. ROGERS.
2. Communication on the General Principles of Analytical Mechanics, by Prof. B. PEIRCE.
3. A paper on the Classification of the Animal Kingdom, according to embryological data, by Prof. LOUIS AGASSIZ.

The Association, having thus far transacted its business as a single body, now resolved to adjourn till to-morrow at 12 o'clock, to allow time for the organization of the Sections, and the transaction of such business as might be found appropriate to each Section.

WALTER R. JOHNSON, *Sec'ry.*

September 21, 1848.

SECTION OF NATURAL HISTORY, GEOLOGY, &c.

The Section was called to order by W. C. REDFIELD, Esq., Chairman of the Association, when Prof. LOUIS AGASSIZ was selected to preside, and Dr. R. W. GIBBES chosen Secretary.

Prof. AGASSIZ, on taking the Chair, suggested the propriety of electing on each day a chairman for the succeeding day, in order to

allow freedom of action in attending either Section, which was unanimously agreed to.

The Chairman reported the recommendation of the Standing Committee, as to the order of business for the day, which was adopted, and the Section proceeded to take up the first paper submitted, which was upon—

The Theory of the Geological Action of the Tides. By Lieut. C. H. Davis, U. S. N.

It was presented and read by Prof. Peirce, of Cambridge University, who prefaced it by a few remarks on the general principles of the theory,—the object of the paper being to exhibit the action of the moon, as tending to alter the figure of the earth.

By a study of the tidal currents on the north-eastern coast of the United States, Lieut. Davis has been led to the discovery of a connexion between the ocean tides and currents, and the *alluvial deposits* on its borders and in its depths. The connexion is thus traced:—the direction and velocity of the tides at any place where these deposits exist, that is where the ocean is freighted with matter held in suspension, decides the form, amount and locality of the deposits. The direction of the tides is different at different places, but the result of their action is to produce certain uniform or similar formations; and it was the observation of this which led Lieut. Davis to the introduction of a tidal theory into Geology, the object of which is to develop the laws by which aqueous deposits (of the sea), made during periods of quiet action, have been regulated, and to show that such laws must always have operated, except when suspended or controlled by the violent changes that mark certain geological epochs. Lieut. Davis applies these principles of tidal action, to explain the cause of those great sandy deposits on the north-eastern borders of this continent, as well as those at the bottom of the Bay of Biscay (the Landes of France), and in the North Sea (Holland, &c.).

Following in the steps of that theory, which aims at accounting for the changes of previous ages by causes now in operation, and recognising the controlling influence of the ocean in producing some of these changes, Lieut. Davis calls the attention of geologists to the fact, that the ocean has been subject to dynamical laws as permanent as its existence, and in their action no less regular than permanent. He traces the results of these laws first in the local features of the New England coast, where they are exemplified upon a small scale;

and afterwards applies his reasoning to those vast alluvial deposits, which form conspicuous features in the geography of the world.

Going behind the actual geological period, Lieut. Davis believes that he can discover satisfactory proofs in the geological maps of the United States, by Mr. Lyell, that the successive deposits of the cretaceous, tertiary, and post pliocene periods (including in this last the *stratified* drift only), have been made in obedience to the tidal laws. He hopes, by these laws, to explain the geological peculiarities of the great plains of North and South America, and the deserts of other countries. And reasoning back from a principle of conformation discoverable in the banks, shoals, hooks, bars, &c., &c., of the present time, he thinks it not improbable that the character of the tidal and other currents of the earlier ages may be developed.

Although Lieut. Davis has given to his views the name of a "Tidal Theory in Geology," yet he does not limit his inquiries to tide currents alone, but embraces in his theory all those oceanic currents which produce similar effects. Thus the sandy formations on the coast of the gulf of Mexico, and the geographical distribution of the coralline detritus in the China sea, and elsewhere, in connexion with the tides and currents of the particular region under notice, do not escape his attention.

Lieut. Davis announces as one of the discoveries resulting from his investigations, that there is an intimate relation between tides and deltas. River deltas are only formed where there is little or no tide. Where there is a tide of a positive and regular character and action, the conflict between tidal and river currents gives birth to estuaries, and it is expected that the limit of the bay form will be found to depend upon the limit of action of the tides.

Lieut. Davis is employed in preparing a paper, in which the dynamical action of the tides is explained, and the facts and observations upon which his theory is founded, are stated in detail. This paper will be accompanied with the necessary illustrations.

It would occupy too much space to enter here into these details. The attempt would leave them necessarily incomplete.

An interesting discussion ensued on Lieut. Davis' views, in which Prof. Peirce, W. C. Redfield, Esq., and Dr. A. A. Gould, participated.

Mr. Redfield said he was gratified in finding this inquiry entered upon by the able officer connected with the Coast Survey. The subject had attracted his own attention at times, and particularly during a

visit to the low country and shores of North Carolina, in the vicinity of Cape Hatteras, in the year 1840; and he had derived many interesting facts relating to it, from the lake and coast surveys executed by the Government, apart from his own observations. It was his misfortune to have heard but a portion of the interesting essay of Lieut. Davis, and he feared he might be liable to a misapprehension of some of its bearings. For himself, he had been led to conclude, that the great shoals of Newfoundland, George's, Nantucket, and Lagullus, did not owe their origin to the general transporting power of the Gulf Stream, or the current of Lagullus, nor to the direct action of the great tidal wave, in its progress over the ocean. He thought that none of these could hold in suspension and carry over great distances the materials of these shoals, nor exert a moving force upon the ocean bottom which lies beneath their flow. Even if the latter could be true, the materials would undergo a rapid abrasion, from their rolling motion beneath the incumbent waters, and would soon be reduced to an impalpable condition, and scattered abroad over the ocean bed, forming a soft and oozy bottom, such as now results from other abrasions, or as once was the condition of our clay slates, and other analogous formations.

Mr. R. then proceeded to state facts illustrative of the rapid reduction, by abrasion, of the materials set in motion by currents—as on the Hudson river, where, near the city of Troy, the transported detritus of the river shores and bed, consisted of large gravel and worn pebbles, often of the size of the eggs of the goose or hen. Six miles lower, near Albany, the bottom was reduced to a fine gravel, of the size of the different varieties of beans and maize. Next, four or six miles below Albany, it was in the condition of a coarse or fine sand; and long before reaching Hudson, thirty miles from Albany, the channel bottom was found to be of soft materials, such as form the clay beds of the river shoals and flats; and we have no evidence that any sands or coarser materials can be found in the great channel bottom, from thence to the ocean at Sandy Hook, a distance of 130 miles. The bar and shoals near Sandy Hook, like those which obstruct the mouths of other rivers, were derived from the materials of the adjacent beaches, abraded and set in motion chiefly by the waves, and especially in storms, these effects being blended with the action of the local currents. Hence it results, that where the mouth of a river is protected from these effects by points or promontories of rock, we almost invariably find a free and open channel.

The ocean, as has justly been said by Mr. Lyell, was the greatest

of all agents in terrestrial degradation, and by the mechanical action and transporting power of its waves, the materials of its shores are brought under the local influences of the shifting streams of tidal current, and are arranged in bars, island-beaches, and salient points or shoals, which have often a progressive increase on one hand, while undergoing degradation and removal on the other. These effects, as seen at Cape Cod, Block Island, Fire Island, Sandy Hook, Hatteras, &c., may be traced along our whole coast, from Massachusetts Bay to Mexico.

Such were the general views he had been led to entertain, on a subject which hitherto had received but little of that attention to which it was justly entitled; and he trusted that the inquiry now so happily commenced, would be unremittingly pursued, under the favourable circumstances and auspices which were afforded by the general survey of the coast now in progress.

Dr. Gould, in allusion to a statement, that substances are found drifted in opposition to the course of the wind, mentioned a fact, well known to persons resident on the coast, that, when the wind blows off the coast, the waves throw up shells in greater abundance than when it blows in the opposite direction.

Prof. Agassiz adverted to the necessity of comparing the results of all observers, in order to arrive at important data for the guidance of geologists.

Prof. Peirce gave an account of the views of E. Desor, Esq., "*On the Local Distribution of Marine Animals*," an abstract of which has not been handed in for publication.

Prof. Agassiz now interested the Section with his observations—

On the Fishes of Lake Superior.

Prof. A. remarked that it had been his good fortune, the past summer, to have opportunities, in company with several friends, some of whom he now saw before him, to explore the northern lakes, and more especially Lake Superior. His attention had been called particularly to the fishes—a subject which has possessed for him, for many years, very great interest. His object was not so much to draw a comparison between the fishes of the United States and those of Canada, as to ascertain their geological distribution, and to satisfy himself whether they were indiscriminately distributed through all these lakes, or whether there were differences in their identity. Upon

carefully comparing them, he had found that they were entirely different—that there are particular families in some of these lakes, and other families in others, and that they never leave their peculiar localities. There are families in Lake Huron, which cannot be found in Lake Superior, and that those in the latter do not move down to the lower lakes. He is satisfied that these fish originate in the localities in which they are found. It is a singular fact, he remarked, that these families are found in the same relative positions with the fish in lakes of Northern Europe, yet, although they agree, generally, with the European species, in their zoological classification, they are entirely different in structure and variety. The salt waters of Europe contain fish which the salt waters do not contain here; and among the fresh waters, although they generally agree, none are *entirely* alike—showing that there can be no transportation of the different varieties from one country to another, as it would be contrary to the laws of nature.

There is no convenience for the fish of our lakes to travel into the lakes of Northern Europe; and he proceeded to inquire whether these inconveniences were trifling or serious in their character. The subject, however, was too complex to arrive at a definite conclusion. In Lake Huron there are a great variety of fishes, belonging to the Perch family, which he classified under appropriate heads. It is well known, from geographical data, that North America is the oldest continental land known upon earth; and the general ancient character of this country is deeply impressed upon the mind of the geologist. He could not avoid the influence of this feeling when exploring the northern shore of Lake Superior. Is it not remarkable, he asked, that animals now exist, which are "old-fashioned" in their external zoological character, of the same type with those of the antediluvian periods? North America is the only place where the Garpikes live, and the Garpike is the only representative of the period when that fish *alone existed!*

Among the fishes there are two types—one with smooth, the other cerated scales. Those with cerated scales have usually two dorsal fins, but he had found in Lake Superior a new fish, with spines upon the opercular bones; and all the scales are hard and cerated. What has never before been observed in hard scale fishes, this species has, like the Salmon, an adipose, or fatty fin. Here, then, in Lake Superior, we have this old-fashioned fish, upon this old soil! He considered it important to trace our living animals in their relation to the fossils, as also that their geographical distribution should be noticed.

The whole number of fishes he had collected from Lake Superior, was 33, about a dozen of which were entirely new varieties.

Prof. AGASSIZ then read Prof. J. W. BAILEY's paper "ON MICROSCOPIC FORAMINIFERA, FROM THE SOUNDINGS OF THE COAST SURVEY," to decide whether of importance in navigation. He spoke of the figures given, as valuable from being given as seen, and not drawn with any attempt to give them with precision, as supposed to exist.

Dr. S. G. MORTON submitted the following letter from Prof. M. TUOMEY on the results of his observations in the Tertiary region of South Carolina.

Alabama, Sept. 5, 1848.

Dear Sir,—I sent you, some days since, as I once promised, a specimen of A. placenta and T. thoracica, from the Eocene of Wilmington, N. C. Of the former species I think there is doubt, the latter I must leave to you. You may recollect, that on another occasion I sent P. Gregale and T. Harlani from S. C. I intended to have accompanied the specimens with a paper on the Eocene of N. and S. Carolina, but as my report on the geology of the latter State will soon be published, I have thought it better to postpone it for the present, particularly as I am on the way towards St. Stephens, to examine the orbitoidal limestone of that place.

Should this reach you in time, may I ask the favour of you to state to the Geological Secretary of the Assistant Geologists, the following results of my investigations in the Tertiary of South Carolina, provided that you find them of sufficient interest.

1st. That the Eocene of S. Carolina is composed of three well characterized divisions: of these, the lowest in the series is the burrstone formation, consisting of beds of sand, clay, gravel and grit, resting upon the cretaceous beds.

The fossils, which are all silicified, occur in the upper part of the formation, and along the verge of the superincumbent calcareous strata; they are for the most part identical with those of Claiborne, and represent a littoral fauna. The Santee beds, which overlie the burrstone, consist of limestone, marl and green sand, being an extension of the Eocene of Wilmington. These are again overlaid by the marls of the Ashley and Cooper rivers.

2d. Although the fossils of these beds include nearly all of those considered characteristic of the Eocene of the United States; never-

theless, there are among them well characterized cretaceous forms,* such as—

Ammonites placenta,	De Cay,	Loc.,	Wilmington,	N. Carolina.
Trigonia thoracica,	Morton,	„	„	„
Terebratula harlani,	„	„	Santee,	S. Carolina.
Plagiostoma gregale,	„	„	„	„
Ostrea panda,	„	„	„	„
Gryphæa mutabilis,	„	„	„	„

3d. That the fossils of the Middle Tertiary of S. Carolina contain 46 per cent. of recent species; I have therefore referred that formation to Mr. Lyell's Older Pliocene. The proportion of recent species seems to increase from north to south.

4th. That the Post Pliocene contains several forms no longer found living on the coast of the State, but belong to the living fauna of Florida and the West Indies.

5th. That the coast of S. Carolina presents no evidence of very recent subsidence, as stated by some visiters; but that all the phenomena attributed to that cause, are the result of the horizontal changes going on at this moment.

I am, dear sir, with sincere respect,

Yours, &c.

M. TUOMEY.

Afternoon Session.

In addition to the officers chosen for this Section, Dr. A. A. Gould was appointed an additional Secretary, and Prof. Samuel Henry Dickson and Prof. J. Hall were added to the Standing Committee, which now consists of the following members:

Prof. L. Agassiz, Chairman; Dr. R. W. Gibbes, Secretary; Dr. A. A. Gould, Assistant Secretary; Prof. S. H. Dickson and Prof. J. Hall.

The Section then proceeded to business. Several specimens of the tracks of Mollusca upon sandstone were exhibited by Prof. Hall, who gave his views upon them, as evidences of beaches in the Silurian period. From these tracks of the Mollusca, he was led to believe that sea beaches formerly existed far in the interior of the land. He also exhibited and described some fossils from the Onondaga salt group.

* Mr. Lyell admits O. cretacia among the fossils of the orbitoidal limestone of Alabama.

Dr. Leconte expressed his views "*On the Geographical Distribution of Coleoptera,*" which will be published at length in a work now in preparation for the press.

After an animated discussion on the subject of the geographical distribution of animals, in which many members took part, it was moved and seconded, that an election be held for Chairman, when Dr. S. G. Morton was unanimously chosen.

The Section then adjourned to meet to-morrow at 10½ o'clock, A. M.

R. W. GIBBES, *Sec'ry.*

September 21.

SECTION OF GENERAL PHYSICS, &c.

Morning Session.

Pursuant to the order of business, the Section for General Physics met at 10, A. M., in the Chemical Hall of the University.

A programme of business was submitted by the Committee, and the first page read was—

A Report on the Winds of the Northern Hemisphere. By Prof. J. H. Coffin.

After some remarks upon the importance in investigations of that kind, of lengthened periods of observation, for the purpose of eliminating accidental errors, and of extending the field of research over as wide a field as possible, he proceeded to classify the observations which formed the basis of the report according to the regions or countries in which they were taken. These were widely scattered over both continents, and the Atlantic, Pacific and Arctic Oceans, and embraced an aggregate period of over two thousand years, at 550 fixed stations, beside numerous voyages and tours. A more extensive collection, he remarked, than had ever before been brought together for the purpose.

He next proceeded to point out the method he adopted to determine from these observations, the mean course of the wind, which was the same as that by which the traverse of a ship at sea is resolved.

By the aid of extensive diagrams, he then proceeded to establish the fact that between lat. 33½° and 60°, there is a general current

from the west, (or rather from a little to the south of west,) extending entirely round the globe; but that, as we approach those limits, (particularly on the south,) it gradually loses its decided character, and at the limit all trace of a fixed direction disappears, the current at any place being controlled entirely by local influences. This he illustrated by a separate diagram of the winds at Augusta, Geo. After passing this limit, on the south, he showed that a current from the opposite direction sets in, which, as we go south, gradually assumes a more decided character till we come fully within the limits of the trade-winds. He alluded, in passing, to a peculiarity in the winds west of the Mississippi, between lat. 30° and 40°, as explaining the tracks of storms in those regions.

North of lat. 60°, he showed that there are indications that the strong current that comes down from the north, in the polar regions, veers toward the west, thus establishing a third system, which breaks up at about lat. 60°.

The observations taken at various places, in Russian and British America, Norway and St. Petersburg, in Russia, were alluded to as indicating this fact.

After having gone through with his remarks upon the general course of the winds, he took up the subject of the annual curve which they describe, and showed that while on the eastern coast of Asia it is the same as here, in Europe the curvature seems to be in the opposite direction. Also, that a curvature, physically similar, exists in both the easterly and westerly systems of winds.

He next spoke of the relative force of the different winds, showing how far the general results are modified from this cause.

After remarking that he was compelled, for want of time, to omit several matters embraced in the report, he closed with an expression of thanks to the numerous friends who had aided him in obtaining the necessary data, and whom he mentioned by name, with the kind of aid received from each.

The next subject was A DISCOURSE ON THE FLEXIBLE SURFACE. BY PROF. B. PEIRCE. [Not received.]

Prof. C. U. SHEPHARD, through Prof. B. SILLIMAN, Jr., presented a "REPORT ON METEORITES," which has been published in the *American Journal of Science and Arts*, November, 1848, p. 402.

A paper was now read—

On the Volatility of Potassa and Soda, and their Carbonates. By Profs. W. B. Rogers and R. E. Rogers.

This communication detailed the results of numerous experiments, proving that these materials have a much higher volatility than has hitherto been recognised by chemists, and exhibiting the comparative rapidity with which they evaporate when exposed to the heat of the alcohol lamp and that of the blowpipe.

When potassa, soda, magnesia and lime, were similarly heated and weighed, from time to time during the process, it was found that potassa was most volatile; soda came next; magnesia was much less volatile than soda; and lime so greatly inferior, in this respect, to magnesia, as to require a long continued high temperature to cause a sensible loss. The carbonates of potassa and soda presented the same order, and nearly the same degree of volatility as the bases themselves.

Two separate modes of experiment were adopted. In the one, the substance in solution was dropped in small quantities, on a slip of platina foil, so as when dried to form a delicate tache on the metallic surface. This tache was subjected to the heat of a spirit lamp, by holding the foil in the flame, or again to the stronger heat of this flame, urged by the mouth blowpipe. The change in the tache, in each case, was tested by its alkaline reaction, on delicately prepared turmeric paper.

The second method, a weighed quantity of the alkali, or other material, was exposed to the strongest heat of the table blowpipe, and the evaporation marked by weighing at stated intervals. The following extracts, from the register of experiments, will exemplify these methods and their results.

I. *Experiments with the Tache.*

The tache, in each case, was formed from one, two or more drops of the particular solution used; so that all the taches in each series were exactly equal.

1. Tache of *Potassa* on platinum foil having a strong alkaline reaction, heated by the mouth blowpipe for 2″, was entirely dissipated. Its place on the metal presented *no trace* of alkaline reaction. By repeated trials it was found that at this heat a

merely momentary exposure was sufficient to destroy the reaction of such a tache.

2. A similar *Soda* tache, treated in like manner, required a somewhat longer time for its dissipation.
3. Tache of *Carbonate of Potassa*, having a very strong reaction.
   a. Heated over lamp for 10″; reaction much reduced.
   b. „ „ for 15″; reaction entirely destroyed, and tache dissipated.
   c. Ignited intensely, for a moment, by blowpipe: reaction destroyed whenever the ignition was complete.
   d. Ignited intensely for 2″; reaction in every case destroyed.
4. Tache of *Carbonate of Soda*, having a very strong reaction.
   a. Heated over lamp for 10″; reaction reduced to about $\frac{1}{3}$.
   b. Ignited intensely by blowpipe for 10″; reaction very slight.
   c. Ignited for 30″; reaction and tache entirely gone.
5. *Tache of Magnesia.* This was formed by evaporation of a solution of carbonate of magnesia in carbonic acid water. Until ignited, therefore, the tache consisted of carbonate of magnesia.
   a. The simple tache gave strong alkaline reaction.
   b. Ignited gently over lamp for a moment; reaction slightly increased.
   c. Ignited gently for 10″; reaction reduced to two-thirds.
   d. Ignited intensely by blowpipe for 10″; reaction about 1-20th of (*a*).
   e. Ignited intensely for 30″; reaction about 1-50th of (*a*), but still very distinct.
6. *Tache of Lime.* Formed as the preceding, in the first instance a carbonate.
   a. The reaction of the simple tache extremely faint, scarcely perceptible.
   b. Ignited over lamp for a moment; reaction very intense.
   c. Ignited intensely by blowpipe for 10″; reaction scarcely, if at all diminished.
   d. Ignited, as above, for 30″; very slightly diminished.

II. *Experiments by Weighing.*

1. *Carb. Potassa.*

   1 grain of pure, thoroughly dried carb. potassa, in platina capsule, exposed to intense ignition, over table blowpipe, lost in 1′, 0.2 grains. In a little less than 10′, it was entirely dissipated.

2. *Carb. Soda.*
   1 grain of pure, perfectly dry carb. soda, ignited intensely over the table blowpipe for 10′, lost 0.35 grains.
3. *Magnesia.*
   1 grain, similarly treated, lost in 1′, only 1-100th grains. In 10′, its loss was 8-100 grains.
4. *Lime.*
   1 grain of lime, similarly exposed, for 10′, exhibited no sensible loss.

Reference was made to the bearings of this determination upon chemical analysis. First, as furnishing the means of recognising the presence of the alkalies and the alkaline earths in mineral substances, such as the felspars, hornblends, epidote, &c.; and, secondly, as indicating the probable large loss of the alkalies of vegetable matters, through the intense heat used in converting them into ashes.

Allusion was made also to the almost entire absence of the alkalies in the ashes of anthracite and other coals, as dependent upon the intense heat operating in their combustion; and experiments were adduced, to show that the coal, prior to the combustion, contained alkaline matter in a marked quantity. The volatility of magnesia, as compared with lime, was spoken of as useful in distinguishing between magnesian and calcareous minerals. The application of this property of magnesia to the theory Dolomization was also briefly referred to.

Prof. W. B. Rogers moved that a Standing Committee be appointed for this Section, to arrange the order of business for each day; which was adopted, and the following gentlemen appointed:—

Prof. W. B. Rogers, Chairman; Profs. B. Peirce, B. Silliman, Jr., J. H. C. Coffin and J. H. Coffin.

The Section then adjourned till 4, P. M.

Afternoon Session.

On the reassembling of the Section, Prof. B. Peirce read a paper

Upon certain methods of determining the number of real Roots of Equations, applicable to Transcendental as well as to Algebraic Equations.

Sturm's theorem is perfect for algebraic equations, but is generally too cumbrous for practical use. By stopping, however, at the first,

second, or third of his functions, whenever either of these is sufficiently simple for direct discussion, the number and nature of the real roots of the given equation can be readily ascertained. Prof. PEIRCE illustrated this method by geometrical diagrams, and applied it to some very general cases of algebraic equations.

This was followed by an article—

ON THE ALLEGED INSOLUBILITY OF COPPER IN HYDROCHLORIC ACID; WITH AN EXAMINATION OF FUCH'S METHOD FOR ANALYZING IRON ORES, METALLIC IRON, &c. BY PROFS. R. E. and J. B. ROGERS.

Prof. R. E. ROGERS, in presenting this communication, referred to the received opinion among chemists, that metallic copper is almost entirely insoluble in pure hydrochloric acid, when oxygen is absent; this, which has been made the foundation of an analytical process, first recommended by Fuch, and since by Fresenuis, was found to be inaccurate. By a particular apparatus, in which carbonic acid gas, in one case, and hydrogen gas, in another, was made to flow into the space above the liquid and metal, so as effectually to exclude the atmosphere, it was found that continued boiling caused the copper to be dissolved in marked quantity. Even when exposed to the acid at ordinary temperatures, the atmosphere entirely excluded, it was found that, after a prolonged time, the metal underwent partial solution, bubbles of hydrogen were evolved, and the dichloride of copper was formed. The Professors R. regard these results as clearly proving the incompetency of Fuch's method to afford accurate results.

The following paper was then read:—

ON SOME PHYSICAL PHENOMENA DEPENDENT UPON THE PROGRESSIVE MOTION OF LIGHT. BY PROF. STEPHEN ALEXANDER.

After adverting to the recognised effect of the annual aberration of light, and that which is ordinarily termed planetary aberration, the author more particularly explained the dragging of the shadows of the earth and other planets, first previously noticed by himself in a communication to the American Philosophical Society. Prof. A. then proceeded to the consideration of the case in which light passed through the transparent envelope of a body in motion, and observed that inasmuch as the theory of undulation required that the ether should be possessed of inertia, and the inertia of our atmosphere must be incomparably greater than that of the ether, it would seem to follow that the velocity of the earth's atmosphere, due to its annual motion, must be impressed upon the light of the sun and stars in the

passage of the same through the atmosphere, and thus produce an aberration which, in so far as the earth's motion was concerned, would be the opposite to that which actually exists. As to aberration being, both in mode and measure, what it ought to be, if the earth had no atmosphere, Prof. A. suggested that the explanation of this was to be found in the enormous porosity of the atmosphere; by far the greater portion of the rays, so passing through as to escape the mechanical action of the molecules. When, however, the quantity of atmosphere to be traversed was so great that light must be nearly absorbed, some sensible portion of it might be subject to the influence in question. Prof. A. then referred to the phenomenon of a blue band, seen by himself and others, bordering that edge of the earth's shadow into which the moon entered at the time of the last lunar eclipse, but which was less distinct on the side at which the moon emerged; and showed that these phenomena were consistent with the supposition of such an impulse, accompanied by the dragging of the shadow.

Lastly, Prof. A. suggested that these considerations might have a bearing upon the question of a systematic aberration of the double stars; for if the nature of the envelope of a star were such that its mechanical impulse could be communicated to the light of the star, the theory of emission, with reference to such a star, would be quasi true, and the aberration admissible.

The Section then adjourned to meet at 10, A. M., to-morrow.

B. SILLIMAN, Jr., *Sec'ry.*

September 22.

SECTION OF NATURAL HISTORY, GEOLOGY, &c.

Second Meeting.

The Section met and was organized—Dr. Samuel Geo. Morton in the chair.

A paper was read:—

On the Forces in Nature which Rupture, Contort, Upheave, and Depress the Superficial Strata of the Earth. By Prof. L. J. Germain.

Mr. G. remarked, that it was familiar to every one, that the planes of the terrestrial equator and the plane of the ecliptic, form an angle

of about 23° 28′. There are two motions of the earth, aside from its perturbations, namely, its diurnal revolution around its own axis, and its annual revolution around the sun—each of which revolutions tends to flatten the earth in the direction of their resultant force, in a plane whose direction is intermediate between the direction of the two, operating on the crust of the earth most effectively within the limits of the torrid zone. It was this resultant centrifugal force to which Mr. G. called particular attention, as the substance of his theory of flattening, upheavals, and depressions.

Prof. AGASSIZ favoured the Section with his Observations—

ON THE PHONETIC APPARATUS OF THE CRICKET.

The Professor gave an account of the structure of the Phonetic Apparatus of insects of the order *Orthoptera*, particularly the crickets. He observed that he had been led to these investigations by some interesting remarks upon the wings of the grasshopper, in the Transactions of the Academy of Natural Sciences of this city. The wings of the grasshopper have elastic springs, by which they are folded, when in a state of repose. Prof. A. proceeded to demonstrate the structure of these wings on the black-board. It had occurred to him that the same structure might be found in other insects also, and upon investigation, he had discovered this to be the case. The same general formation is common to all insects of the order of *Orthoptera*, and is a distinguishing feature of all the varieties of that large and interesting group. It is (remarked Prof. A.) by the action and contact of these springs, that these insects produce their music.

Prof. HALDEMAN exhibited a series of specimens of *Phacops Hausmanii.* (Calymene Micrurus. *Green*).

The head and tail of this species are the only portions hitherto discovered; but one of the specimens, now presented, contains the body which has eleven segments.

Prof. HALL added some remarks on the same subject.

Prof. AGASSIZ now gave his views—

ON THE COMPARISON OF ALPINE AND NORTHERN VEGETATION.

His remarks referred principally to the general observations of Humboldt; but many facts observed on the North American continent

were added in confirmation of the general principle. Prof. A. detailed the limits and appropriate regions of various trees of our own country. He remarked that they mostly differ from European species, although analogous in general character. All the walnuts, he remarked, are distinct from European varieties, yet usually resemble them.

Dr. Dickeson now read an extended report on "The Sediment of the Mississippi River," prepared by Andrew Brown, Esq. and Dr. M. W. Dickeson, which is here published in full.

To the American Association for the Advancement of Science.

Gentlemen,—The undersigned Committee, charged with the duty of collecting and reporting to this Association, as many of the facts and characteristics respecting the condition and annual conduct of the Mississippi river, as might be practicable, herein respectfully submit a series of facts and observations in connexion therewith, collected by virtue of the most favourable opportunities of daily observation for the last eighteen years, and continued without intermission with a view to this report, for the last two years of that time, beginning the 1st July, 1846, and ending 30th June, 1848; comprising a series of notations and calculations of the quantity of water at the several stages of elevation and depression, during the river's annual oscillations, between its mean high water and low water lines, together with the quantity of detrital or sedimentary matter with which it is charged, &c.

The quantity of water embraced in the respective calculations, being agreeable to the mean of time and velocity for high and low water; and at every intermediate space of twelve inches, between the mean high water and the mean low water line, the several quantities calculated for the respective stages, constituting the aggregate of water passing the point of observation; hence, necessarily, the annual discharge from the valley of the Mississippi in the Gulf of Mexico.

Your Committee have deemed it highly essential to this report, that it should be accompanied by a sectional diagram of the river at the point of observation, accurately delineating the configuration of its bed and banks, the better to enable the Association to perceive the value of the data from which the respective calculations and quantities have been deduced.

In furtherance of such a view, a sectional diagram is herewith submitted, together with a book marked "A," in which is contained the specific quantities of square feet of water at the several stages of ele-

vation, as per diagram, which constitutes the basis of calculations for the several times and quantities at the respective stages. The method for arriving at such facts, will be better perceived on referring to the book, in which is noted all the important data for obtaining the accurate and aggregate quantity of water, discharged by the river for each twelve months; such being the mean of the above specified two years. Without such a detailed system of observations and calculations, as your Committee have thus adopted, they might have failed to find, with the same accuracy, the required quantities; for they were unable to perceive by what other method the facts could be more surely approximated; and should the tediousness of this report, together with the necessary inspection of the accompanying documents, be a tax on the patience of this Association, we flatter ourselves that a compensation will be found in the unquestionable authenticity of the facts submitted.

It will readily be perceived, that although this report embraces but one point in the whole of this long river, all the descending waters must necessarily pass each point on their way to the ocean; consequently, the river at this point must be in corresponding depth and velocity, to enable the waters to effect such a passage.

The velocity assumed for the waters at the several stages of elevation, as per calculation in book "A," and which constitutes so important an element in these calculations, is not that of the central current, but is the mean of the lateral quantity obtained by many and repeated experiments and computations, which gave an amount very sensibly less than the central current, and which is variable under varying circumstances; but it may be observed, that while these sensible variations of current exist in the lateral expansion of the river, we have been altogether unable to detect any appreciable difference of current in the vertical quantity, and that, too, after having made many experiments with regard to that particular. What may be regarded as almost, if not altogether, conclusive evidence of that fact, is the circumstance that it is no very unusual thing for tall trees to float down the deepest part of the river, in a perfectly perpendicular attitude, caused by their butt ends being of greater specific gravity than water, while their tops or small ends are so buoyant as often to project many feet above the surface of the water. Such vertical trees are, at times, transported with the same velocity as the surface current where they exhibit themselves; and while such trees are thus floating in a perpendicular attitude, it often occurs that their lower ends are in such close proximity with the bottom, as to come in contact with its pro-

tuberances, which throw them down at such angles as often to make their tops disappear below the surface, until they have got over the obstruction; and when such is the case they at once erect themselves as before. We have had many opportunities of witnessing the descent of vertical timber, and frequently when their coming in contact with the projecting parts of the bottom manifested itself; and it is otherwise well ascertained, that the bed of the river projects many protuberances above its general surface, some of which rise to and even project above extreme low water—as, for instance, that represented on the diagram—so that they very much endanger river navigation at the low water stages. They constitute obstructions, with which trees descending in a vertical attitude often come in contact, and by which they are so tossed and agitated, that no particular form of any tree could make its projecting part seem to be perpendicular, while its submerged portion was inclined, conformable with a current which was not uniform in its whole depth; for were the under current more sluggish than that on the top, the lower end of the tree would be correspondingly impeded in its progress, so as to give to the tree an inclined attitude; but the observed circumstance shows that there can be no such inclination, for when agitated by striking against the projecting parts of the bottom, they turn freely round in every direction, and present their several sides to the direction of the current, without any seeming preference as to position.

Our observations induce in us the conviction, that in a descending aqueous current, there is no appreciable difference of velocity throughout the vertical quantity; and unquestionably for the reason, that the superincumbent pressure urges forward the under stratum to the point of least resistance, with the same velocity that it may itself have acquired.

Governed by the foregoing considerations, and estimating quantities by the method adopted in book "A," your Committee have found the aggregate annual quantity of water discharged by the Mississippi river, to be 14,883,360,636,880 cubic feet, or 551,235,579,143 cubic yards. 101.1 cubic miles = $101\frac{1}{10}$.

Now the fact being notorious, that the Mississippi river is the only visible outlet for all the surplus waters of that vast valley, through which it passes on its way to the ocean, there are involved considerations of no little importance; for the Mississippi valley has been found to contain an area very little, if any, short of fourteen hundred thousand square miles.

What then is the relative difference between the quantity of rain

water falling annually in this valley, and that discharged by the river.

We find by examination of the meteorological register of the late Dr. H. Tooley, of Natchez, kindly furnished by his family, that the mean quantity of rain water, which falls annually at Natchez, is between fifty-five and fifty-six inches; but, as such is the quantity for the southern extremity of the valley, it may be regarded as over estimating, if taken for an average of the whole area, we will therefore assume the mean quantity to be 52 inches, and then we will have for the whole valley, 169,128,960,000,000 cubic feet, which is about $11\frac{3}{8}$ or 11.3636 times the quantity which is discharged by the river.

There can be but two ways by which this immense quantity of water can make its escape from the valley; one is by the course of the river, and the other by evaporation. Hence, we perceive that there is but one relative portion of this quantity passing off by the river, for every $10\frac{3}{8}$ parts which are exhaled into the atmosphere, or $\frac{8}{91}$ parts are carried off by the river, and $\frac{83}{91}$ parts by evaporation.

Thus we arrive at a fact of the most momentous importance to the planting interests of Louisiana and Mississippi; for it will be at once perceived, that the more exhalations are promoted, the less liable will the low or bottom lands of these two states be to the periodical inundations by the river.

If it be asked, by what process it is expected that evaporation can be promoted, over such an extensive area as the Mississippi valley, so as visibly and permanently to affect the planting interests of the above named States, the answer will be found in the fact, that the process has been, and is now, in the most rapid and successful progress, and of that kind which is the best calculated to produce so desirable a result, viz: the clearing of such large portions of the valley of its forests for the promotion of agriculture, and the consequent exposure of the lands to the action of the sun and winds, the very best promoters of the evaporating process, particularly on a large scale.

It will not be difficult to perceive the great difference there must necessarily be in the evaporation from a surface of country exposed to such action, and that which is covered by the primitive forests and their almost impervious undergrowth, through which neither sun nor wind can penetrate but with difficulty.

So rapid is the progress of this increased exposure, and its consequent evaporating tendency, and so visible has been its effects on the Mississippi river, that we may hazard the assertion with safety,

that there is not now, by twenty or twenty-five per cent., as much water passes down the Mississippi as there was twenty-five years ago; for at and prior to that time there were annual inundations of many feet, and long periods of submergence of almost all the bottom lands, from the bluffs or highlands on one side of the river bottom to those on the other side. Such lands were at that period in a great measure accounted valueless; and to such a degree, that but little or no hopes were entertained of the practicability of their redemption by any artificial means, that is, on any general scale; but such has been the diminution in the annual quantity of water discharged from the valley, that those lands have been progressively and rapidly redeemed from overflow, until very great portions of them are now in the very highest state of cultivation, and with but comparatively slight assistance from art in the way of embankments, and these such as could not have been at all available against the overwhelming effects of floods, and the length of time of their continuance; for then there were annual inundations, both deep and expansive, of the waters, over almost all the bottom lands; but now the river seldom rises to the same elevation as formerly, and when it does, it is of much shorter duration, and the waters are almost exclusively confined to the channel of the river, in place of being spread over almost all the bottom lands the whole spring and early part of the summer.

These changes in the quantity of water discharged are so progressive, that they fail to excite general notice; so that the lands which are at one time considered to be of little or no value, are subsequently taken up, occupied and improved with success, without any consciousness that such an important change is in progress, the opinion being prevalent, that in nature there can be but little change, and that the annual quantity of water descending the river at one period must approximate very nearly to the quantity that descends at every other, there being no visible cause for suspecting it otherwise.

All the advantages are progressively but rapidly extending themselves while the cause remains unsuspected or overlooked, but none the less sure. As a further evidence of the altered condition of this river, we may mention the circumstance, that in former times, and in the spring of the year particularly, steamboats ascending or descending the river were detained about half their time by dense fogs, while now hardly any such obstructions prevail; so that steam-packets succeed in making their trips to an hour, with no fear of such retardation.

Assuming that the diminution of the waters will continue in some-

what the same ratio they have recently done, the time cannot be very far distant when all apprehension from inundation will have in a great measure passed away.

We will further remark, as an evidence of change, that the quantity of floating timber or drift wood passing annually down the river, has diminished in a far greater ratio than that of the water; for the drift now passes at longer intervals, in smaller quantities, and for much shorter periods than it once did, so that the aggregate quantity cannot now be over fifty per cent. of that which formerly passed down. The cause of the diminution is similar to that of the other, and is to be found in the circumstance of the banks of the river and its tributaries being denuded of their timber for steamboat fuel, and as a process preparatory for agricultural pursuits, the immediate banks being the location at first usually selected for such purposes.

Second Section of Report.

This section will be found to contain a further statement of facts in relation to the quantity of solid matter, with which the waters of the Mississippi are annually charged, together with its effects in the formation of lands, or filling up of depressions.

In order to arrive at these required facts the following methods were adopted:—First, a series of glass vessels, of a cylindrical form, were procured, to one end of which (that being the section of a cylinder), there was attached a tin tube of the same cylindrical diameter as that of the glass vessel to which it was attached; in this tin tube, immediately above its junction with the glass cylinder, there was inserted a small brass cock, by which the tin tube could be conveniently discharged of its contents at pleasure, without causing any disturbance to the contents of the glass vessel below; this attached tin tube was in length, above its lower opening, 48 inches.

This tube was charged with water from the Mississippi river, and that water allowed time to deposit its contents into the glass vessel below; that being accomplished, the water was drawn off, and the tube recharged by more water from the river, each particular change being carefully noted; this process was successively repeated for the different conditions and stages of the river's height and velocity, which very materially affected the quantity in suspension. Thus, by a succession of such changes and dischargings of the tin tube, amounting in all to four hundred and eighty-four times, or, in the aggregate, to a column of water of 1936 feet, there was deposited a column of sediment or solid matter of 46½ inches (such column of sediment

herein submitted), enclosed in three of the respective glass cylinders above named, and in which the same was deposited from the water in the attached tin tube. But this sediment still seems to evince some slight disposition for further settlement, and with a knowledge of its formér habits, we would say that it would be unsafe to decide on its final quantity being more than 44 inches; greater certainty would have been obtained by giving it another year; but as the most of it has been long collected, it cannot now, we think, shrink to less than 44 inches. Assuming that, therefore, to be the true quantity, and the product of a column of river water of 23,232 inches, it necessarily follows, that as 44 is to 23,232, so is the quantity of solid or sedimentary matter contained in the water, to the volume of the river; or, in words and figures, the mean proportional quantity of sediment to the river, is as 1 to 528. We have, therefore, already ascertained the quantity of water annually discharged by the Mississippi river to be 14,883,360,636,880 cubic feet, there must then be deposited from that quantity of water, 28,188,053,892$\frac{1}{6}$ cubic feet of solid matter.

In collecting the test-water, from which the above 44 inches of sediment was obtained, much care was taken to procure it from that part of the current where it was sufficiently agitated to prevent, in any measure, a subsidence of such matter as should be held in suspension. It was fully decided, after many trials, that there was no sensible difference of quantity contained in any part of the water throughout its whole depth, or from the top to the bottom of the river, provided it was in the main current, for where agitation was equal and effective, there also the suspension of sedimentary matter was found to be equal.

There can be no question but that much matter, in the character of coarse sand and gravel, is transported by the river current; of the quantity of this your Committee could have no possible opportunity of estimating the value, or even ascertaining its existence, only that the many sand and gravel bars visible at low water stages of the river are composed, to a considerable extent, of such matter, and they are subject to a perpetual change of position, and consequent tendency of their matter to the river's mouth.

Being in possession of the data, by which may be computed with some approximation to certainty, the effects of the Mississippi deposit in the formation of land, or in filling up the gulf into which it is emptied, we will avail ourselves of such data, and endeavour to present the quantities deducible therefrom. In estimating the delta of the Mississippi we have adopted for it the designation and superficies

assumed by Mr. Lyell, in his investigation on this subject, and will say, with that gentleman, that the delta of the Mississippi river comprehends all that great alluvial plain which lies below or to the south of what, until recently, was the first branching off or highest arm of the river, called the "Achafalaya." This delta is computed to contain a superficial area of 13,600 square miles. In deciding on the depth of this quantity we will adopt that which was assumed by Prof. Riddell, on this subject, and say that it is of the average depth of one-fifth of a mile, or 1056 feet, inferred from that being the average depth of the Gulf of Mexico, from the Balize to the point of Florida.

We find, by computation, agreeably to the above data, that it would require a quantity not less than 400,378,429,440,000 cubic feet, or 2720 cubic miles of solid matter to constitute this "delta." Having ascertained the quantity of solid matter, annually brought down by the Mississippi river, to be 28,188,083,892 cubic feet, which would be equal to one square mile of the depth of 1056 feet in 381⅓ days, or one cubic mile in five years, eighty-one days, it therefore follows, that it would require a series of 14,203⅘ years for the river to effect the final formation of the present "delta."

We are not disposed to consider that great alluvial plain, stretching with the river, from the above designated delta, as far up as Cape Girardeau, in Missouri, as any part of the delta proper, nor can it ever have been any continuation of the Gulf of Mexico, or arm of the ocean, as is usually supposed; the evidences are vastly against any such conclusions, inasmuch as the diluvium which constitutes the highlands bordering on each side of this alluvial plain, by its general distribution, would have been equally deposited in such gulf or arm of the sea, which, in reality, could not have been the case, for the river has excavated through this diluvium, and exposed it in many places, resting on what is evidently of another formation; and such is not only found to be the case at the base of the diluvial hills, but the same formation is found also to constitute the bed of the river at many other points, detached for very considerable distances from any high lands. On reference to the reported diagram, right hand side, will be perceived the true position of the formation and base of the diluvial bluffs, which are in depth, to high water line, at this point, 179¼ feet; and it will be seen, that they extend almost as low as the bottom of the river, making their whole depth about 220 feet.

There have been no excavations or perforations through the alluvial matter in any part of this vast plain, excepting those which have been effected by the river itself; but it would seem that it does not

exceed, in any part, the depth of the river, whose high-water line may be regarded as the average level of the plain, and from that to the bottom of the river is about 117 feet. This bed of the river is a substance of entirely different character from the composition of any part of the diluvial bluffs.

In building a breakwater, your Committee had occasion to quarry much of this substance, and found it to possess all the characteristics of a well formed rock, which had to be acted on by wedges, crowbars and pickaxes to effect its reduction; but, on exposure to the weather, it gradually pulverizes and becomes an adhesive blackish clay, but very different from the tenacious clay of the alluvium or diluvium.

From the above observations, should they truly represent the character of the plain, the influence is irresistible, that the Mississippi river has excavated the whole of this valley, and denuded the upper tertiary of all its incumbent diluvium, from the high lands on one side to those on the other, of which every indication is confirmatory; and there are very evident traces of the river having occupied every part of this bottom, for its ancient beds are found at intervals in the form of lakes and depressions throughout the whole extent of it.

The superficial area of this great valley has been found to be about 16,000 square miles, bound by high lands on either side, ranging from 50 to 250 feet above the level of the plain. Should this space, therefore, have been reduced or excavated by the river, as we assume, it must have transported the diluvial matter, and caused it to form part or portion of its delta. Now assuming the average height of the high land above the plains to be 150 feet, we will therefore obtain 454½ cubic miles, or 66,908,160,000,000 cubic feet of matter, as its proportional contribution, in the formation of the delta, the balance required being 333,471,269,440,000 cubic feet, to be derived from the reduction of other lands, the two sources being to each other as 1 to 5.98; or, by giving another expression to the same quantities, there is, in the "delta," 2720 cubic miles of matter; 454½ of which would be derived from the diluvium in the excavation of this valley; the other portion would consist of 2265½ cubic miles, to be derived from other sources, or the reduction of other lands.

We have now traced this great river through a period of 14,204 years; but how it was occupied before that time, or what was the condition of the country over which its waters passed, is more than we can safely venture to say; but, on particular examination of the bluffs, or cliffs, which bound its present plain, it will be very difficult to resist the conviction, that the river has had great agency in depositing

the upper or loamy stratum, which varies from a few feet to upwards of fifty feet in thickness, in all of which stratum there is abundance of land and fluviatile shells, such as those now found in the present deposite from the river. On receding from the river bluffs, we find that the clay, or other diluvial matter, which underlies the loam bed on the top of the bluffs, crops out, very clearly indicating an ancient valley, or depression in the diluvium, which constituted the more primitive bed of the river, on whose sloping flanks, and, more particularly, in their depressions, this fluviatile matter, or ancient alluvium, was deposited.

Any variation of its character, from that which is now deposited, may be very readily accounted for, by the varying condition of the ground over which the waters have had to pass, for it may be safely inferred, that they then had to make their way over the face of a country very different in its character from the present; for they have gone on, progressively, to abrade the high lands of their lighter matter, and to cut deep channels in the valleys, down through the more primitive rocks, whose detrital matter must necessarily assume a somewhat different character from that which it formerly had, particularly that on the top of the bluffs. We have found the age of that deposite to be not less than 14,204 years, through all of which time the waters have been actively engaged in changing the face of the country, and transporting 2720 cubic miles of its matter to a far distant location.

The above may be said to comprehend all the required particulars with respect to the waters of the Mississippi river or its deposite, which your Committee are at this time prepared to submit; yet they cannot permit this opportunity to escape them of continuing their remarks with respect to some incidental circumstances in connexion therewith, or inferrible therefrom.

We found, in the incipient stages of the depositing process, a very decided want of uniformity to take place in the deposition of the sedimentary matter in the glass tube, which, in place of settling level, was, on the contrary, found to be settling in such a manner as to give it a very inclined upper surface. The cause of this unexpected peculiarity was inquired into, and at once suspected to proceed from the unequal distribution or action of light; one side of the tube being more exposed to that influence than the other. To verify this conjecture, the tube was turned round in an opposite direction to that influence, when the low side not only recovered itself, but very soon had an inclination upwards; and, as often as the turning round was resorted

to, the same effect was produced; for most sediment would persist in settling on the dark side of the tube, that being least agitated by the action of light. To render the cause of this phenomenon a fact no longer to be doubted, a slip of black paper was procured, in width about half the circumference of the glass cylinder, and to one side of which it was applied in order to exclude the light from that side, while it had free access to the other; the result was as anticipated, for it caused a very much increased deposite on the sides shaded by the paper.

This variation, or inclined settling, progressively decreased as the lighter part of the tube, through which the particles had to fall, became shortened by its filling up with sediment.

The above fact is here noticed under the supposition that it may be something new, and that, by possibility, there may be connected with it, something which may be found beneficial to science; for it is, to say the least of it, a very strong evidence of the action of light upon delicate substances.

Having early perceived that this report would exhibit a series of results, varying very much from those obtained by Prof. Riddell, on the same subject, and reported to this Association in 1846, we have, on that account, been the more careful that accuracy should attend all our proceedings, with respect to it. This has likewise enhanced our desire to possess the Association with as many of the details of the methods adopted to insure the proper results as should be the best calculated to show the confidence that was to be placed on the unquestionableness of their nature. By this report, the mean annual discharge of water, by the Mississippi, is 14,883,360,636,880 cubic feet; the Riddell report gives 11,108,275,200,000 cubic feet. This difference may be accounted for by the great uncertainty there must be in obtaining any correct mean where conditions and velocities are so changeable; at least there cannot certainly be the same dependence placed in quantities got by such a method, as there may be in that more mathematical one adopted for this report. But the difference in the two estimates, with respect to the solid matter held in suspension by the waters, may well seem the most extraordinary that can be conceived, in any two estimates for the same thing. This report gives to the solid matter, in bulk, in comparison with the bulk of water, as 1 is to 528, the Riddell report gives it as 1 to 3000. Why there should be any thing like this difference may seem difficult to determine, and is well calculated to abate the confidence to which either report may be entitled. Agreeably to the Riddell report, it

would require a column of water, 250 feet in height, to deposit 1 inch of sediment; were that so, it would have required that our tin tube should have been filled 1750 times, in order to have produced the quantity of sediment herewith submitted; in which case we would have contented ourselves with obtaining a much smaller quantity. According to this report, there is 1 inch of solid matter deposited from a column of water of 44 feet. Every one who has travelled on the Mississippi, and seen the turbid condition of its waters, will be able to decide between these two reported quantities. We will here remark, so as to account for the discrepancy, that if the waters of this river are not continually agitated by the force and turbulence of the current, the suspended sediment very soon subsides considerably below the surface; and at, or opposite the Mint, in New Orleans, is below almost all the wharves, and most of the shipping. Where the waters are very still, and where deposition takes place to a very great extent, so as to make much land, after the water has so parted with a portion of its sediment, it passes down past the Mint without being but very slightly agitated, the principal current being altogether on the other side of the river, and but very little on that side where Prof. Riddell procured the test-water for his experiments, according to his own statement.

Thus, when the true conditions are known, the discrepancies will cease to be so surprising; but it is certainly very unfortunate that they should have existed at all, or that the water opposite the Mint should have been collected as an example to establish the proportional quantities of solid matter held in suspension by the waters of the Mississippi.

One more remark and we will have done: It has been elicited by the circumstance of perceiving a very decided disposition in the procured sediment to continue settling down into a more compact condition by virtue of molecular affinity, or the arrangement of its constituent particles, and consists in accounting for the often noticed circumstance, that in a large excavation made for the Gas Works, at New Orleans, 18 feet deep, there were found embedded innumerable stumps of trees, buried at various levels, in an erect position, with their roots attached, implying the former existence there of fresh water swamps, covered with trees, over which the sediment of the Mississippi river had been spread during inundations. The site of the excavation is only about nine feet above the level of the sea; therefore the lowest of the embedded stumps must be nine feet below that level. Now this need not be at all surprising, when we reflect that

the first growth of trees, in this region, must have taken root on the surface of new-made ground, deposited from the Mississippi, and that ground, or deposite, of the depth of 1056 feet, with a continued tendency to settle into a more compact condition by incumbent pressure, molecular affinity, and arrangement of particles, as we found it to be the case with the settling of our sediment. Every year's sinking down, in this case, would be fully compensated by every year's deposite, for the lower it at any time sunk the longer it would be subject to inundation by the subsequent overflow, so that this sinking and compensatory process would go on together, and continue about equal, which well accounts for the circumstances of former forests being now found so far below the level of the sea.

Should this subsidiary process be now, in any measure, incomplete in that region where the City of New Orleans now stands, and which we indeed very much question, in that case the compensatory equivalent being cut off by embankments, or the levying out of the waters, the time may, by possibility, yet come, when the ground on which the City of New Orleans now stands, may sink to a level with the ocean, or even below it; at least, should there be any further settlement at all, it must, in the same ratio, approximate that level; for the final adjustment of particles in a deposite of 1056 feet deep, that will preclude any further settling, may be expected to take an immensity of time.

ANDREW BROWN,
M. W. DICKESON,
Committee.

Table of Calculations made use of in the foregoing Report.

1. Quantity of water discharged by the Mississippi river, annually, 14,883,360,636,880 cubic feet.
2. Quantity of sediment discharged by the Mississippi river, annually, 28,188,083,892 cubic feet.
3. Area of the delta of the Mississippi, according to Mr. Lyell, 13,600 square miles.
4. Depth of the delta, according to Prof. Riddell, 1056 feet.
5. The delta, therefore, according to 3 and 4, contains 400,378,429,440,000 cubic feet, or 2720 cubic miles.
6. According to 2, it would require, for the formation of one cubic mile of delta, 5 years 81 days.
7. For the formation of one square mile, of the depth of 1056 feet, 1 year $16\frac{1}{5}$ days.
8. For the formation of the delta, according to 2, 3, 4, $14{,}203\frac{4}{5}$ years.

9. The Valley of the Mississippi, from Cape Girardeau to the delta, is estimated to contain 16,000 square miles, of 150 feet deep; it therefore contains 66,908,160,000,000 cubic feet, or 454½ cubic miles.

Prof. GERMAIN gave some views of his connected with this subject.

On motion of Prof. W. R. JOHNSON, it was recommended to the Association that thanks be presented to the Committee for the very able manner in which they have discharged their duty in this matter, and that the report be published in full as early as practicable.

Dr. R. COATES moved that this Section adjourn to meet to-morrow at 10½ A. M., which was agreed to.

R. W. GIBBES, *Sec'ry.*

September 22.

SECTION OF GENERAL PHYSICS, &c.

Second Meeting.

The Section met at 9. A. M. The following papers were read:

ON THE FUNDAMENTAL PRINCIPLES OF MATHEMATICS. BY PROF, STEPHEN ALEXANDER.

Prof. ALEXANDER remarked, the object of all scientific research was *truth;* a term too valuable to be misunderstood, and yet too general to admit of a ready definition. He proceeded, however, to characterize it, in some of its various aspects, observing that while each is applicable to its own object, that is true in mathematics, which, under the existing system of things, is supposable; that is true in physics, which, under the existing system of things, has been permitted to exist; that is true in matters of taste which is consistent with the laws of beauty, deduced from the observation of things actual; and that is true in morals, (in the highest and best sense in which it is good,) which is consistent with what is found in the GREAT SOURCE OF ALL GOOD.

He next proceeded to state that mathematics had not to do with things, but the relations of things, and it was sufficient that those relations should be supposable; and that the certainty of mathematical

reasoning rested upon the fact, that those relations could be more readily understood and completely defined, than the properties of the things themselves.

He stated, moreover, that these were constituted relations, and not mere figments of the human mind; the things which we had to deal with, being made, in certain respects, not merely *what* they were, but *as* they were. Thus, that two bodies did not occupy the same space, and that it was true that one day of the week must follow another, were not true because his audience and himself might think so, but because the *Creator had made them so.*

He next commented upon the general term which was used to designate that to which mathematical reasoning was applicable; viz. *quantity;* and said, that in so far as mathematics had to do with it, it was that which admitted of the distinction of greater or less. Moreover, quantities were of the *same* species when each *in itself* exceeded its less in the self same respect in which the other *in itself* exceeded its less; whatever might be true of the boundaries or limits of either. Thus, a straight line and a curve were of the same species, since each exceeded its less in length; so, also, an hour and a minute were of the same species, since each exceeded its less in duration.

He remarked, that the only point of resemblance between quantities of different species, was to be found in the fact, that the distinction of greater and less was admissible in the case of every species; and hence it was possible to compare the *ratio* of two quantities of one species, with that of two quantities of another species.

He proceeded to the more special consideration of the two great relations of things, time, and space; remarking, that space might be described, as that wherein there was room for the separate existence of material substances; and duration, as that wherein there was room in another sense for the separate, and therefore successive, occurrence of events.

He next commented upon the nature of *zero;* showing that it implied the absence of the species of quantity which happened to be the subject of investigation, and not the absence of every other. That, thus, the surface which bounded a solid quantity, was not *somewhat* in the same sense in which the solid was somewhat, viz. in the property of occupying space, but only *somewhere*, viz. where the solid terminated, and space met it; the space without met it, though that surface was still *somewhat in superficial extent.* That the line which bordered the surface was not *somewhat* in this last respect, but only *somewhere;* though still *somewhat in length.*

Lastly, that the point which terminated the line was not *somewhat in any respect*, but only *somewhere;* viz. at the end of the line; and that the same was true when a point was otherwise situated; e. g. the centre of a sphere.

He remarked, that an instant also existed as the limit of duration; e. g. the midnight with which one day terminated and the other began; but this existed not *where* the one ended and the other began, but *when;* or such a limit was not somewhere, but, if there were such a word, *somewhen*. That rest, or the *zero of motion*, existed *when* and *where* a body came to rest, and that *shadow* existed *when and where* light was absent.

He moreover considered the subject of infinity, and distinguished three sorts of infinity.

He remarked that he should designate a quantity as *absolutely* infinite, if it were so great as to be destitute of *any boundary or limit;* and gave the only two recognised examples of this, viz. boundless space, and that duration which is made up of Eternity, Past and Future. Eternity past was that which found its realization in the Divine Pre-existence, and Eternity future was to be found in the endless duration of the same; and nothing less than the combination of both of these, nothing short of it, constituted the absolute infinity of duration.

He moreover remarked that he should designate a quantity as being *specifically infinite*, if it were just as boundless as those last described, but in certain respects only. He gave as examples:—

1. A straight line without termination in either direction from a point which might be assumed in that line, such a line would be specifically infinite; viz. in length.

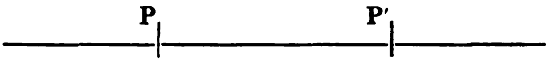
2. A surface without border which would be specifically infinite; viz. in length, breadth, and superficial area. He drew the conclusion, moreover, that an interminable line which was not straight throughout, must be longer than that which was perfectly straight, since the former not merely extended through space in its length, but intruded somewhat upon the breadth of space.

He next remarked that he should designate a quantity as being in comparison with another, *relatively infinite*, if its ratio to that other were too great to be estimated; that in this sense alone could we speak of an infinite *number* of things, or of an infinitely great number in the abstract. The like must be true of velocity, and also of mere mechanical force.

He next considered the subject of motion as applicable to mathe-

matical quantities, and gave some illustrations showing, that when bodies moved they forsook the positions in space which they at first occupied, and that the position occupied by the centre of gravity, or any specified point of reference with regard to the body, was in like manner left behind, and a new position in space be so situated, as to be the centre of gravity, or point of reference of the body; both the space first occupied and the positions left behind having, themselves, no motion. He therefore designated the motion of a *mathematical* point, as being a pleasant fiction, and said that, were it otherwise, a point, which was *nothing*, might, by motion, produce a line which had *length*.

He next supposed a point (P) to be assumed in an interminable line, and remarked, that all that portion of the line on the one side of the point, must be regarded as being *in effect* the half of the line, and all on the other side as being, in effect, the other half. But if a new point (P′) were assumed in the same line at any finite distance from



the other, the two portions, one on the one side, and the other on the other side, must, as before, be regarded as being, in effect, the halves of the line; though all the intervening portion of the line (P P′) had (at the new point of division) been taken from the one half, and added to the other. Hence, any finite straight line must be regarded as *good for nothing*, in comparison with a straight line interminable in only one direction; or if the line thus interminable were used as the measuring unit, its ratio to any finite straight line must be represented by $\frac{1}{0}$. Any other finite straight line, however great or however small, must in like manner be represented by *zero* in comparison with the same measuring unit; and the ratio of the one finite quantity to the other, be therefore represented by $\frac{0}{0}$. Hence $\frac{0}{0}$ was a symbol of indeterminateness. In this case that indeterminateness would be absolute.

Prof. A. also remarked with regard to another common case, in which the value of $\frac{0}{0}$ might enter; viz. $\frac{P(X-a)^m}{Q(X-a)^n}$, which $= \frac{0}{0}$ when $X = a$; that, in this case, the numerator and denominator both were reduced to zero, because the multiplier in each case vanished, so that *no process* of multiplication was possible; and there was, in each case, *absolutely no result:* insomuch, that vanishing fractions might, in this point of view, be rather termed vanished fractions.

[Prof. A. next, incidentally, spoke of the reason why the radius in

the investigation, of analytic trigonometry must be regarded as positive when measured from the centre outwards to any point on the circumference.

He adverted for this purpose to the method employed for the determination of the position of a point in space, showing that the distance from the origin in one direction must be regarded as positive, to whichever of the three axes reference was employed, and that a negative distance could, in every case, be obtained, by measuring backward from (P) the farther extremity of the opposite distance (P M), an extent (P P′) greater than the positive distance, and thus passing in the opposite direction to the other side of the origin.

He next supposed the angles made by some (or all) of the axes with each other, to be so increased that those axes should all be brought into the same plane. The three directions from the origin outward would thus be found to be positive, while the opposite directions must be regarded as negative. As, moreover, any number of groups of axes, three in each, might thus be clustered around the same origin, all directions from the centre outward must then be regarded as positive, and the contrary negative.]

Prof. ALEXANDER then resumed the comparison of finite quantities with the infinite, and in manner as before, proceeded to show that if a plane were supposed to extend through all space, all the portion on the one side of this plane must be regarded as being, *in effect*, the half of all space, and that all on the other side, as being, in effect, the other half. The like would, however, be true if another such plane were to extend through space parallel to the first; though to what before constituted the one half, would be added all the space between the two planes, and the same subtracted from the other. Hence, reasoning in the manner as before, we must conclude that this intervening space, though boundless in some of its dimensions, must be regarded as *good for nothing* in comparison with the half of all space; i. e. the half in the sense already described.

For like reasons, any finite portion of time must be regarded as *nothing*, in comparison with either eternity past or eternity future; and thus we might, in some humble measure, discern how, in view of a mind which could grasp the whole, "a thousand years" would be "as one day, and one day as a thousand years."

He lastly considered the question—whether, if the visible creation were annihilated, space would still exist, and concluded that we had not sufficiently accurate ideas of such a state of things to determine with regard to it; but insisted that, in any event, space could not

exist, independent of the GREAT FIRST CAUSE, in whose existence, as it was, and is, and is to come, was to be found *the one, the absolutely necessary truth, and that all others were contingent, just so far as He had made them so.*

ON THE ZODIACS OF THE ASTEROIDS. BY PROF. J. S. HUBBARD.

Prof. HUBBARD, of the Washington Observatory, stated to the Association that he was then engaged in computing the Zodiacs of the Asteriods. The term Zodiacs, as here applied, he defined as referring to the zone or belt within which are included all possible geocentric positions of the particular asteroid in question: and the object in thus determining these belts was to facilitate researches into the past history of these remarkable bodies; since in most cases, the question of identity of a missing star, with any asteroid, may be settled at once by a simple inspection of the Zodiacs. The method of computation was that proposed by Gauss in the Monatliche Correspondenz, Vol. X., from which Prof. H. quoted the plan of the analytical investigation.

ON THE DECOMPOSITION OF ROCKS BY METEORIC AGENTS, AND ON THE ACTION OF THE MINERAL ACIDS ON FELDSPAR, &c. BY PROFS. W. B. AND R. E. ROGERS.

[An abstract of this memoir may be found in the fifth volume of the American Journal of Science and Arts, p. 401.]

ON THE DECOMPOSITION OF ROCKS BY METEORIC WATER. BY PROFS. W. B. ROGERS AND R. E. ROGERS.

In presenting this communication, the fact was stated, that only one or two observations have hitherto been made by chemists, to test, in a direct and conclusive manner, the power of water, at ordinary temperatures, to decompose rocky substances; at the same time, the general fact of such a decomposition appears to have been assumed in explaining the disintegration of mineral masses, and the conveyance of inorganic ingredients into the substance of plants. The experiments have applied to all the principal crystalline minerals, containing alkalies and alkaline earths, amounting to nearly forty species—to the principle aggregates, such as granite, gneiss, &c.—to the different varieties of glass—and to various kinds of coal and wood.

These experiments were also of two kinds with each specimen—the one with pure distilled water; the other with water charged with carbonic acid. The mineral, or other matter, being reduced to a very fine powder in an agate mortar, was in small quantity mixed with the liquid, and transferred to a filter of purified paper. One or more drops of the percolating fluid, received on a slip of platinum was gently evaporated to dryness, and the tache resulting was then examined by delicate test paper. In all cases, the residuum from the carbonic acid water, was greater than from the other; but even in the former, with most minerals, a decided alkaline reaction was obtained. By heating the tache gently for a short, and then a longer time, and again strongly by the blowpipe, unequivocal proof was furnished of the presence of potassa or soda—or of lime or magnesia. The liquid from calcareous or magnesian minerals, becomes milky after heating—the tache from the potash and soda minerals, as for example, the feldspars, lost its alkaline reaction by the first contact of the blowpipe flame—that from the lime was greatly augmented by the first calcination, in consequence of the removal of carbonic acid, and continued intensely alkaline after a prolonged exposure to the heat. The tache furnished by magnesia minerals, such as the serpentines, was much impaired in alkalinity by igniting, but continued to present a decided reaction with the test paper, after long exposure to the heat. In this way the behaviour of the tache was shown to be capable of furnishing a useful auxiliary means of extemporaneous qualitative analysis.

Some experiments were introduced, in which these effects were properly exhibited by powdered glass, mica, and feldspar.

The attention of chemists was especially invited to these phenomena, as having very important bearings, not only upon the decomposition of rocky masses by the action of the percolating rain, but the subsequent introduction of various crystalline minerals in the rifts and cavities of the strata, and as indicating the necessity of some new and better method than that commonly employed, for determining the amount of alkali present in vegetable or other organic matters.

Experiments were also cited, disproving the opinion which appears to be received among chemists, that the feldspars, hornblends, &c., are entirely unacted upon by sulphuric or hydrochloric acids. By exposing these materials in fine powder to prolonged digestion in the acid, even at common temperatures, a partial solution was found to result. Thus 30 grains of potash feldspar, by digestion for twelve hours, in hydrochloric acid, at temperature 60 degrees, lost nearly

one grain, and the liquid furnished chloride of potassium with chloride of aluminum.

On the Absorption of Carbonic Acid by Leibig's Dilute Solution of Phosphate of Soda. By Profs. W. B. Rogers and R. E. Rogers.

Prof. R. E. Rogers gave the results of experiments made by himself and brother, upon this subject, by a dilute solution of the common phosphate of soda, containing one grain of the salt to 100 grains of water, referred to in Leibig's work, entitled, "The Chemistry of Food." By a peculiar form of apparatus, furnishing very accurate results, this saline solution has been found to absorb a far larger proportion of carbonic acid, than is attributed to it by Professor Leibig. The amount given by the latter is 138 per cent., while that found by the Professors R. for the solution at 60 degrees, was 207.9 per cent., and at blood heat, 153.05 per cent.

The Section then adjourned to meet to-morrow at 9 A. M.

B. SILLIMAN, Jr., *Sec'ry.*

Friday, September 22, 12 o'clock.

The Association met agreeably to adjournment; William C. Redfield, Esq., President, in the chair.

The Secretary of the Section of General Physics reported, that the Section had been organized by the appointment of Prof. Joseph Henry, Chairman, and Prof. Benjamin Silliman, Jr., Secretary, and that a Standing Committee had been appointed for said Section, composed of—

Prof. Wm. B. Rogers, Prof. Benjamin Peirce, Prof. James H. Coffin, Prof. B. Silliman, Jr., and Prof. John H. C. Coffin.

Also, that Prof. R. E. Rogers had been appointed Assistant Secretary of the Section.

He also reported, that the scientific proceedings which had taken place in the Section yesterday and this forenoon, were as follows:—

Prof. B. Silliman, Jr.. reported for the Section on General Physics—

That Prof. Coffin had read a Paper on Winds, being a report on

that subject, compiled in conformity to an appointment made at a previous meeting of the Association of American Geologists.

That a paper had been read by Profs. W. B. and R. E. Rogers, on the Volatility of Potassa and Soda, and their Carbonates.

That Prof. Alexander had read a paper on Aberration of Light.

The Secretary of the Section of Natural History reported the organization of that Section by the appointment of Prof. Louis Agassiz Chairman; Dr. R. W. Gibbes, Secretary; Dr. A. A. Gould, Assistant Secretary: and, in addition to the above, Dr. S. H. Dickson and Prof. James Hall, as members of the Standing Committee.

Dr. Gibbes also reported the scientific proceedings had in the Section of Natural History, during yesterday and to-day, as follows:—

Theory of the Geological Action of the Tides, by Lieut. C. H. Davis, U. S. Coast Survey.

On the Local Distribution of Marine Animals, by M. Desor.

On the Fishes of Lake Superior, by Prof. Agassiz.

On the Foraminifera, derived from deep sea soundings, in the Coast Survey, by Prof. J. W. Bailey.

On the Tracks of Mollusca, as an evidence of Beaches in the Silurian period.

On the Geographical Distribution of Coleoptera, by Dr. J. L. Leconte.

The reports made by the Secretaries of the Sections were severally adopted.

A communication from the officers of the University at Cambridge was received, inviting the Association to hold its next annual session in that city. Action upon the adoption of the proposal was postponed, and the invitation was referred to the Standing Committee.

A motion was made, that the proceedings be published in the form of a Journal, which proposition, after a discussion, participated in by Messrs. S. W. Roberts, Dr. M. W. Dickeson, Prof. W. R. Johnson, Prof. L. Agassiz and others, was finally referred to the Standing Committee.

The Chairman of the Standing Committee nominated for membership in the Association Dr. J. B. Lindsley, of Nashville, Tenn., and the vote being taken, the nomination was confirmed.

A communication was received from the Philadelphia Athenæum, inviting the members of the Association to visit the rooms of their Institution during the session, which was, on motion, accepted.

On motion, adjourned, to meet again at half-past 4 o'clock, this afternoon.

Friday, September 22, 4½ o'clock.

The Association met pursuant to adjournment, WILLIAM C. REDFIELD, Esq., President, in the chair.

The Chairman of the Standing Committee reported a resolution accepting the invitation of the officers of the University at Cambridge to hold the next Annual Meeting of the Association in that city, and recommending that the 14th of August, 1849, be the day set apart for such assembling. After a short discussion, and an explanation of the reasons for adopting that as the period of meeting, the resolution was adopted.

Lieut. MAURY, of the U. S. Navy, then presented a communication ON THE WINDS AND CURRENTS OF THE OCEAN, of which the following is an abstract:—

Lieut. MAURY reminded the Association of its having appointed a committee, in 1844, to represent to the Secretary of the Navy the importance of the information which our public cruisers might collect with regard to the Gulf Stream, and other subjects of general interest.

It was scarcely necessary to say, that the Secretary who had signalized his administration of the Navy by the most enlightened and liberal policy, with regard to the true interests of science, and of which the National Observatory and Dead Sea Expedition were monuments, gave to the representations of the committee the most respectful consideration.

It was owing to an impulse which was thus given, that Lieutenant MAURY had been enabled to carry into effect a favourite idea, long entertained, of constructing a series of charts, which should give to each navigator, who would consult them, the benefit of the experience of all who had gone before him, as to the winds and currents, and the like, in any given part of the ocean.

Charts of the North Atlantic, upon this plan, were exhibited before the Association. They are so constructed as to show at a glance the prevailing winds, currents, temperature of the water, &c., for every month in all parts of the ocean. The characters or symbols for the winds are so contrived, that they show, at once, both the direction and strength of the wind.

To obtain the results thus exhibited, involved immense labour:

many thousand old log-books had been overhauled, and the records of each, as to winds, temperature of the sea, variation of the compass, and force and set of currents, compared with all the rest. The results, so far, were of high interest and great value. They show that the trade-winds, in the North Atlantic, blow with more regularity on the American than on the African side of the Atlantic, owing, probably, to the fact, that in the latter case the sands and deserts which heat and rarify the air, are to windward; while, in the former, they are to leeward. It was also shown that the so called north-east trade winds prevail more from the northward, on the American than they do on the African side of the ocean, and that calms are much less frequent on this, than on that side of the ocean.

By a peculiar manner of grouping the statistics, with regard to winds, these charts have led to the discovery of a region near the equator, and extending midway the Atlantic, from the shores of Africa, within which, instead of a N. E. trade-wind, as there is between the same parallels, in other parts of the ocean, there is a regular system of monsoons.

The usual route of vessels, in the Atlantic, from the northern to the southern hemisphere, passes through this anomalous region. To get to it, vessels from the United States are in the habit of sailing nearly across the Atlantic, passing down near the Cape de Verds, and so on, under the impression that when they get within it, they should find the winds favourable for a S. W. course, to enable them to recross the Atlantic, *with fair winds*, and so pass down the coast of Brazil; whereas, the chart showed, that of all courses, the winds were there most unfavourable for a course to the southward.

This discovery, with others, developed by the chart, has led to the suggestion of a new route, nearer, and through a region of better winds, to the equator. Seven vessels, which had tried this new route, had returned their log-books to the National Observatory, at Washington; and the result was, that the mean of the seven passages was 11 days, or about 25 per cent. less than the average length of passage by the usual route.

The manner in which the charts were cut up, by the tracks of vessels, enabled him to speak confidently as to the existence of a number of *vigias*, and other dangers of doubtful position, which disfigure our best and most accurate general charts. Many of these were pronounced to have no real existence.

Many curious and interesting facts were also exhibited concerning the temperature of the ocean. Among these was pointed out, off the

shores of South America, between the parallels of 35° and 40° S., a region of the ocean, in which the temperature is as high as that of our own Gulf Stream; whereas, in the middle of the ocean, and between the same parallels, the temperature of the water was not so great by 22°.

This region of warm water was noted for gales of wind. It is the most stormy that the charts, which are only partially completed for the South Atlantic, had as yet indicated.

The habitual neglect of the use of the water-thermometer among navigators, generally, had been felt as a matter of serious inconvenience in this undertaking. It is one of the most useful instruments in navigation. All mariners, especially those sailing out of New York and New England should bear in mind the importance of ascertaining the temperatures of the ocean; for New York owed her commercial prosperity, in a great degree, to a discovery that had been made with the water-thermometer.

At the time when Dr. Franklin, with this instrument, discovered the Gulf Stream, Charleston had more foreign trade than New York and all the New England States together. Charleston was then the half-way house between New and Old England. When a vessel, attempting to enter the Delaware or Sandy Hook, met a N. W. gale, or snow-storm, as at certain seasons she is so apt to do, instead of running off for a few hours into the Gulf Stream, to thaw and get warm, as she now does, she put off for Charleston or the West Indies, and there remained till the return of spring, before making another attempt.

New York now has more direct trade in a week than Charleston has in a year. Perhaps Dr. Franklin, with his water-thermometer, and Jeremiah Thompson & Co., with their packet-ships, may be regarded as the two most powerful agents of the many concerned in this revolution in the course of trade. A beautiful instance this of the importance and bearings of a single fact, elicited by science from the works of nature.

The frequent and general use of the water-thermometer, by navigators, seems to be the only means by which we may hope to arrive at a proper knowledge of the aqueous circulation of the globe—of the currents and isothermal lines of the ocean.

The Secretary of the Navy had authorized copies of these charts to be given to every navigator who would return to the National Observatory, according to form, an abstract of his voyages. Several thousand sheets of the chart had already been distributed upon these

terms; and there are now engaged, in all parts of the ocean, hundreds of vessels making and recording observations for this work. Never, before, was such a corps of observers known; and never, before, could the commercial marine of any country boast of such a body of navigators as are those of America.

The importance of simultaneous observations, in all parts of the ocean, cannot be overrated. They would enhance the interest of the work, and greatly add to the value of the results. The field is as wide as the ocean, and there is room in it yet for multitudes of labourers, thousands of whom are wanted for simultaneous observations. The work is not exclusively for the benefit of any nation or age; and, in this view of the subject, it was suggested, whether the states of Christendom might not be induced to co-operate with their navies in the undertaking; at least so far as to cause abstracts of their log-books and sea-journals to be furnished to Lieut. MAURY, at the National Observatory, Washington.

A communication was received from Dr. HARE, inviting the members to a conversazione at his house on Monday evening next, which was, on motion, accepted.

Prof. W. B. ROGERS offered the following resolution:—

Resolved, That a Committee of five be appointed to address a memorial to the Secretary of the Navy, requesting his further aid in procuring for Lieut. MAURY the use of the observations of European and other foreign navigators, for the extension and perfecting of his charts of winds and currents; which resolution was adopted, and the following appointed said committee, viz. Prof. W. B. ROGERS, of Virginia, Prof. JOSEPH HENRY, of Washington, Prof. B. PEIRCE, of Cambridge, Mass., Prof. JAMES H. COFFIN, of Easton, Pa., and Prof. STEPHEN ALEXANDER, of Princeton, N. J.

On motion of Prof. AGASSIZ, seconded by Prof. W. B. ROGERS,

Resolved, That the thanks of the Association be tendered to the Committee on the Sediments of the Mississippi river, for the report this day rendered to the Geological Section, and for the able and satisfactory manner in which the duties of said Committee have been performed.

Resolved, That it is highly desirable that immediate steps be taken to publish entire the series of valuable observations and important deductions made by the above Committee.

On motion adjourned, to meet at half past 7 o'clock this evening, at which time it was announced that the annual address, by the Chair-

man of the last annual meeting, would be delivered, and that communications would be made by Prof. B. PEIRCE and Prof. L. AGASSIZ.

Friday, Sept. 22. Evening general and public Session.

The Association met agreeably to adjournment, President WILLIAM C. REDFIELD, Esq., in the chair.

The President announced the annual address, when Prof. WM. B. ROGERS rose and delivered an extremely interesting and spirited discourse, which we regret that ill health has prevented his writing out for publication.

Prof. BENJAMIN PEIRCE next read a paper "ON THE GENERAL PRINCIPLES OF ANALYTICAL MECHANICS." [Not received.]

Prof. LOUIS AGASSIZ followed with a paper "ON THE CLASSIFICATION OF THE ANIMAL KINGDOM." [Not received.]

The Association then adjourned to meet to-morrow morning at 4 o'clock, P. M., in the Chemical Lecture Room of the University.

WALTER R. JOHNSON, *Sec'ry.*

September 23.

SECTION OF NATURAL HISTORY, GEOLOGY, &c.

Third Meeting.

At the meeting this morning Prof. W. B. ROGERS was called to the chair.

Prof. AGASSIZ then proceeded to give his views upon—

THE TERRACES AND ANCIENT RIVER BARS, DRIFT, BOULDERS, AND POLISHED SURFACES OF LAKE SUPERIOR.

He said that the remarks he was about to present upon this subject, were more of a practical kind, than made in reference to the theory which he thought likely to explain these phenomena. He would insist upon the facts, in order, if possible, to arrive at just conclusions. The hypothesis he presented was derived from an investigation of the Swiss glaciers. He went on to explain the identity of these phenomena with the appearance of those regions where the glaciers occur.

It was his conviction that two distinct causes had produced these

appearances upon Lake Superior. He had observed evidences of the action of water, but there were other phenomena for which he claimed another agency, viz. the *terraces* around the lake; which he believed indicated the shores of former water basins, showing different levels of the lake—some of these being at its present margin, others higher and higher—giving evidence of former elevated stages of the water. Hence he had little doubt, that the relative level between dry land and water had changed, to the amount now existing between the highest terrace, and the margin of the lake, which is some three hundred feet. He here illustrated his position by the black-board. He thought they presented evidences of paroxysms. Let me, he said, give a few illustrations.

He then proceeded to represent, by diagrams, successive gradations of cliffs and beaches, until we come to the present actual margin of the lake, showing that the lower basin was formed by the action of water upon the earlier deposits of matter. . It is now a question whether these changes of level have been produced by a subsidence of the water, or an upheaval of the land. Facts have been brought forward to show that the water has sunk, but Professor A. inclined to the theory, that the land has risen, caused by a paroxysm or upheaval. It would be difficult, he thought, to account for the action of the water, in producing these basins, three hundred feet above the level of the lake, if there had been no change in the formation of the land. If the water had had a free outlet, as now, through the Sault St. Marie, he did not see how it was possible for the water to have risen so high as the summit of the highest of these beaches. Hence, he leaned to the geological theory of the upheaval of the land.

There was another point to which he wished to refer—the appearance of the muddy drifts and polished surfaces of the lake. He could not see that the polished surfaces were owing to the action of water, for it is characteristic of the action of that element, on a great extent of land, to groove or channel out all the softer portions, by which means, the harder portions of the veins were left projecting. But here, all were ground down equally to one uniform level.

We have *scratches* upon this surface, indicating pressure from *above*, and it was his impression, that these phenomena of the North have been produced by causes identical with those which have produced the glaciers. If we can find whether these glaciers move always in an inclined plane, or by some other cause, we shall have settled a great point.

Professor A. then went on to define the climatic differences which

occur in different regions of our own and other countries, with the alternate changes of freezing and melting—or larger and smaller masses of water; and these changes he thought would be sufficient to move any masses of snow and ice over the flattest surface—even over hills and mountains, if the mass possessed sufficient momentum.

Now where do we observe these scratched and polished surfaces? On the tops of mountains, and the highest hills; yes, on their very summits. He then attributed these effects to the agency of *ice*—although, to a great extent, there are other phenomena which must arise from the action of water.

Prof. AGASSIZ also made remarks upon—

THE BLACK BANDED CYPRYNIDÆ.

He observed that the *markings*, (the black bands,) usually considered so characteristic of the species, go for nothing, when we find that these bands are only observable in the young, and that they are gradually obliterated.

He next proceeded to say, that it was necessary to separate from the family of Cyprynidæ, those which he denominated *Cyprynodonts*. He illustrated his meaning by the black-board. The difference was —that the former had an additional tubercle behind the cerebellum; but in the Cyprynodonts, we have a simple brain without the tubercle.

The next subject was a few observations by Prof. AGASSIZ, on the

MONOGRAPH OF GARPIKES.

He said, it would be recollected, that in the Pliocene, or New Red Sandstone of Connecticut, great numbers of fossils have been found, in various stages of preservation. He had thought it expedient to examine monographically the present existing species of fishes, which closely resemble these fossils. The only types living, which bear a particular resemblance, are the Garpikes. These have been distinguished by several different species, but he had divided the genus *Lepidostesces*, (the Garpikes,) into two types—the sharp-nosed and the flat-nosed. Yet he thought he had found three species, in this investigation, of the former, and also two or three of the latter, or flat-nosed. The details of these investigations, he continued,

would be of very little interest. He hoped, however, that they would soon be sufficiently developed to bring the attention of paleontologists to bear upon the subject. He referred to the structure of the *brinoids*—that is, those star fishes which, in their earlier stages of life, rest upon a stem. He demonstrated his meaning by the black-board. The star fishes, he said, when full grown, show apparently a quite different animal, caused by the formation of loose plates, arranged variously on the surface, and we notice a calcareous centre, whence the different rays diverge. The only difference discoverable was, that instead of these plates forming a series of loose particles, (as in the young,) they are united, in the full-grown animal, into hard developed rays, and the creature assumes its permanent form of a single star, instead of its former *branched* appearance.

Dr. R. W. Gibbes next presented a remarkable fossil, which he desired to refer to Prof. Agassiz for examination. Prof. A. being thus called upon, said he thought it extremely interesting, and that it seemed to bear a close resemblance to the great group of cartilaginous fishes, and thought it likely to indicate a new family, intermediate between the Saw fish (Pristis) and the Cestracionts. The locality, in the far west, is not certainly known, but it is most likely from the *cretaceous*.

Mr. T. Green exhibited and described some new forms of *Rhamnus lanceolatus*.

Prof. Hunt read a paper "On Acid Springs and Gypsum Deposits of the Onondaga Salt Group," which has been published in the American Journal of Science and Arts, March, 1849, p. 175.

Prof. J. C. Booth next presented a paper—

On the Physical Geography and Geology of the Northern Portion of the State of Mississippi. By Richard Bolton, Esq.

The region between the Mississippi bottom and the Tombigbee valley, presents ranges of hills of nearly uniform height, from 80 to 150 feet above the adjacent water-courses, the principal streams being about 18 miles apart. It may, therefore, be regarded as nearly level land, denuded by these water-courses. The water-courses have an upper or dry bottom; a lower wet bottom, subject to overflow in winter, and heavily timbered, and are usually from 1 to 200 feet wide. The valleys and broader hill-tops offer rich arable land to the

planter. Few rocks are visible on the surface, and gravel is rare; hence the streams have neither falls nor rapids.

Two lines, drawn from north to south, crossing the streams, one at the distance of 50 miles from the eastern boundary of the State, and the other 60 miles farther west, will divide the upper part of Mississippi into three belts, which may be denominated the calcareous, arenaceous and alluvial formations. The first embraces the Tombigbee valley and Prairies, including a small portion of the carboniferous series, in the N. E. corner of the State; the second, the hilly region, where sand-rock occasionally appears, and through which the water-courses, flowing west, suddenly turn south, and enter the third, or the Mississippi bottom.

1st Division.—A low ridge, striking off westward from the carboniferous hills, sweeps around the head of the Tombigbee valley, and turning south, bounds this valley on the west, and is the source of the streams flowing through the other divisions. To the east of the Tombigbee river the soil is red, and no rock is visible; nearer to the river, and partly on its west side, the soil is sandy and gravelly, and may be ascribed to the detritus of the carboniferous rocks in which the branches take their rise.

West of this, and reaching to the ridge just mentioned, is the Prairie region, of slight elevation, but sufficiently undulating for drainage, characterized by its rich, black soil, varying from 2 to 20 feet in depth. Commencing near the north limit of the State, and with an average width of 18 miles, it strikes south, about 200 miles, and then turns eastwardly into Alabama. It is underlaid by *Rotten* or Prairie limestone, which, near the town of Aberdeen, in N. Mississippi, has a thickness of 1 to 200 feet; but 12 miles further west, and in S. Alabama, attains a thickness of 900 feet, or more. It appears to dip slightly towards the south. The limestone is white, soft and amorphous, and crumbles readily on exposure to the air; where it appears on the surface it constitutes a "bald Prairie," destitute of herbage. Springs are rarely found issuing from this rock; and wells dug down to it, merely collect the rain-water, which, by percolation through the black soil, has a strong and disagreeable taste. To obtain pure water, artesian wells are sunk through this rock to the subjacent sand, and the water then rises to within 40 or 50 feet of the surface. In similar wells, in S. Alabama, it rises even to 20 feet above the surface.

To the west of the Prairie region lies the ridge first mentioned, which, with its spurs, has an average width of 10 miles. It is com-

posed of a red loam, 25 feet deep, lying upon sand of 30 feet thickness; and this, again, reposing upon a hard, light gray limestone. The rock contains about one quarter sand, burns readily to lime, and seems to be almost wholly composed of gryphæa. It also contains an abundance of echinus and spatangus. The region abounds with springs of pure water, issuing from the sand above the limestone. Spurs, running south-east from the ridge, diminish in height, and occasionally show the rotten limestone near the surface. Their uniformity of slope is remarkable. The streams run south-east, and, in crossing them from south to north the ascent, to the summits of the spurs, is very gradual, but then the descent is very abrupt to the next stream. The springs occur chiefly on this abrupt slope.

2d Division.—Following the streams issuing from the red loam ridge, in their west course, the first strip, of some 8 miles width, is a kind of valley between the red ridge and the following sandstone ridges. Its soil is a grayish, sometimes sandy clay, and mostly covered with post-oak, and unproductive, excepting in some rich black bottoms. It seems to be on a level with the hard limestone, is destitute of springs; its streams are sluggish, and water can rarely be obtained by digging.

The next belt, of 10 miles width, consists of broken, intricate, sandy ridges, frequently capped by red sandstone, and covered with pine trees. Fresh water springs abound in the hollows; a feeble chalybeate is sometimes found, and, more rarely, a sulphur spring. Petrified wood and coal (lignite?) occur, the latter in thin strata.

The following belt, of 20 miles width, is arable, and characterized by long, level, dry sandy valleys, by numerous springs and free flowing streams of pure water. The soil is a light vegetable mould. The tops of the ridges, more or less level and fertile, are composed of a light coloured loam, of 10 to 20 feet depth, resting on a quicksand of unknown depth (over 100 feet).

The last belt has an ash-coloured soil, approaching that of the Mississippi bottom, with compact sand and some gravel, and a sandstone rock, showing itself here and there. Springs are scarce, and wells on the ridges are often sunk to the level of the Mississippi river. The ridges of this belt subside by steep descents of 80 or 120 feet, to the Mississippi bottom.

This *3d Division*, or the Mississippi bottom, extends from Memphis to Vicksburg, 180 miles in length, and is 60 miles wide in the middle, tapering towards each end. Its features are too well known to require description.

The divisions, and even the subdivisions, given above, are not only characterized by their topography, geology and soil; but, as might be inferred by the peculiar kind of forest covering them, hickory, pine, and various kinds of oak. We are struck with the complete and regular system of drainage; the size of the streams and their valleys being always proportional to the extent of country drained. It is evident, from this fact, conjoined with the character of the formations, that the whole has been quietly and simultaneously elevated, and has not materially suffered by subsequent subterranean action. It appears, further, that the Prairie belt, east of the red loam ridge, and the clay belt west of the same, were two marine valleys, and that the remainder of the country constituted a great level, probably with a very gentle southern slope, which, as the land rose, was scooped into hill and valley, with the peculiar features which are its present characteristics.

The Section then adjourned till Monday.

R. W. GIBBES, *Secretary.*

September 23.

SECTION OF GENERAL PHYSICS, &c.

Third Meeting.

On motion, Lieut. M. F. MAURY, U. S. N., was called to the chair.

A paper was read "ON CHEMICAL PHILOSOPHY." By Mr. T. S. HUNT.

This paper has been published in the American Journal of Science and Arts for May, 1849.

A paper was read by Prof. JAMES H. COFFIN "ON THE MEAN VALUES OF DIFFERENT POWERS OF THE RADII VECTORES OF THE ELLIPSE," in which he showed—

1st. That the mean value of their first powers is equal to the semi-transverse axis.

2d. That the mean value of their squares is equal to the square of the semi-transverse axis, plus half the square of the eccentricity.

3d. That the mean value of their cubes is equal to the cube of the semi-transverse axis, plus the square of the eccentricity, multiplied by three-fourths of the transverse axis.

Prof. M. H. Boyè's paper, "ON THE COMPOSITION OF BITTERN," was now read. It has been published in the American Journal of Science and Arts for January, 1849.

The next communication was by Mr. SEARS C. WALKER, through Prof. ALEXANDER—

ON THE OPPOSITION OF NEPTUNE OF 1848.

Mr. SEARS C. WALKER communicated to the Section the comparison of his Ephemeris of Neptune, for the opposition of 1848, with the observations received from Hamburg and Cambridge, England. After applying to the Ephemeris a correction published by Mr. W. in the American Journal of Science, the differences between the computed and observed places of Neptune, are as follows:—

		Obs. R. A.	Eph. Decl.
Hamburg,	July 10,	— 0."06	— 0."83
	11,	+ 2. 51	— 0. 68
	12,	+ 0. 78	+ 1. 86
Cambridge, E.	July 12,	— 0. 39	+ 1. 25
	13,	— 2. 55	+ 0. 07
	15,	— 1. 86	
	Mean	— 0. 26	+ 0. 34

This comparison shows that no correction is needed as yet, either to Mr. Walker's elements of Neptune's orbit, or to the perturbations of the planet, as computed by Prof. PEIRCE.

The following paper was read:—

ON THE TOPOGRAPHY OF THE STATES OF PENNSYLVANIA AND OHIO, &c. BY SOLOMON W. ROBERTS, Esq.

The speaker commenced by saying, that the maps which he exhibited, showed the results of numerous and extensive surveys, which had recently been made. A rail-road is in use from Philadelphia to Harrisburg, on the Susquehanna, a distance of 107 miles, passing over a rolling and highly cultivated country, but not crossing any high ridges. The Pennsylvania Rail-road, now in course of construction, from Harrisburg to Pittsburg, at the head of the Ohio river, will be 251 miles in length, making the whole distance from Philadelphia to Pittsburg, 358 miles. This line crosses the Alleghany mountain at Sugar Run Gap; and from Harrisburg to the base of the mountain, a distance of 133 miles, the line follows the valley of the

Juniata river, and has no grade greater than 21 feet per mile. The curvatures are easy, and the road adapted to high velocities. The mountain is ascended on the eastern side by $12\frac{3}{10}$ miles of a grade of 80 feet per mile, similar to that on the Western Rail-road of Massachusetts. The summit of the mountain is then passed by a tunnel 700 yards long, and the line from the summit to Pittsburg is 106 miles long, with a maximum grade of 52 feet per mile.

The rail-road distance from Pittsburg to Cincinnati will be 330 miles, by the way of Massillon, Wooster, and Columbus, while the distance by the Ohio river is 495 miles, or one half longer than the rail-road; and the rail-road may be traversed in about one-fourth of the time required by steamboats on the river. The rail-road in Ohio for the greater part of its length will traverse the elevated table lands of that state, which are very favourable for rail-road construction.

The speaker described the principal topographical features of the States of Pennsylvania and Ohio, and exhibited a profile of the crest line of the Alleghany mountain for a distance of 44 miles.

In conclusion, he explained that the best and shortest rail-road route from Cincinnati to New York and Boston, passed through Pennsylvania and Philadelphia; and that the same was true of a road from St. Louis. And also that from Cleveland on Lake Erie to New York, the distance by the rail-road through Pittsburg and Philadelphia, will be 80 miles shorter than by the way of Dunkirk and Piermont.

A communication was read on—

Definitions and Discriminations respecting Matter, Void Space, and Nihility. By Prof. Robert Hare.

In what respect does a void space resemble nihility?

Either implies a negation of matter.

In what do they differ?

Space can hold either matter or spirit, nihility neither. Nothing can hold nothing.

Space implies the capacity to hold something, whether corporeal or spiritual, and differs from nihility as a mathematical solid differs from a mathematical point. It is unity in imagination, multiplied three times by any number representing imaginary length, breadth, and thickness, whence results an imaginary solid.

How does space differ from matter?

Matter is capable of any degree of motion, space utterly immovable. Yet to move matter requires a force, and to stop it when in motion, force is requisite. The ability to retain its actual state, whether of rest or motion, has been designated as the power of indolence, "vis inertiæ," and the consequent motive force when in motion, as momentum. Of the vis inertiæ, either as constituting momentum, or resisting it, space is, of itself, as destitute and insusceptible as nihility.

The most minute portion of matter avails as something; the largest portion of space is, per se, equivalent to nothing.

Agreeably to Newton, the atoms of matter are bodies endowed with inertia and gravitation, and each occupying, exclusively, a commensurate portion of space, must have the cohesion necessary to perfect hardness or impenetrability.

According to Faraday and others, it is only necessary to suppose atoms to consist of certain forces acting from their respective centres. But if the supposed centres be mathematical points, they must emanate from nothing, since, physically, a mathematical point has no existence. Out of nothing, nothing can come. Besides, one mathematical point being precisely similar to any other mathematical point, any forces emanating from any one such point, must equally belong to all such other points; and since mathematical points must be infinite, both as to number and as to ubiquity, the universe would be a plenum of inseparable and undistinguishable forces, instead of those which actually exist as appertaining to the various parts of space, casually and transiently occupied by what we call matter. But if, instead of assuming the centres of forces to be ideal mathematical points, it be supposed that the centre of each force is concentric with a minute globular space, which it casually occupies, evidently the globule thus suggested, must have essential attributes of a material particle. It can have no association with the containing space, since space is immovable matter, susceptible of any degree of motion. That of which our planet consists, revolves about the sun with inconceivably swift motion, rendered extremely complicate by diurnal rotation, and revolutionary reaction with the moon. No matter in this planet, or in any other, can exist in the same space for more than an infinitismal of time.

Hence, as neither mathematical points, nor minute spaces existing about them, can be endowed with the forces and properties usually attributed to matter, these forces and properties must belong to some-

thing existing about the centres whence they proceed, and within the spaces whence they act.

When any space is found to be the seat of a force, it follows, inevitably, that there is a material body or bodies therein, which causes it to differ from any like space which is vacant, and of course destitute of any power.

It follows, that whatever is susceptible of motion, and capable of exercising any force or reaction, is a material body according to science, as well as common sense.

It follows, from the preceding reasoning, that spiritual existence involves that of kind of matter; spirits must be endowed with the power of locomotion, and cannot be confounded with mere space, any more than other bodies.

The Section then adjourned.

B. SILLIMAN, Jr., *Sec'ry.*

Saturday, September 23, 4 P. M.

The Association met agreeably to adjournment—President, W. C. Redfield, in the chair.

The Standing Committee reported, that it did not deem it necessary to fill the blank in the 18th rule of the Association; but that for the present year the annual subscription of one dollar, together with such voluntary subscriptions as the members may severally feel able or inclined to add, will, in all probability, be found sufficient to meet the expenses of publishing the proceedings. It was thereupon resolved, that the said blank shall for the present remain.

The Standing Committee also recommended that a complete catalogue of the members of the Association be prepared by the Secretary, and that the Treasurer call upon the members to pay their annual subscriptions, together with such voluntary contributions as they may be inclined to make; which recommendation was adopted.

Prof. Peirce having resigned his place as a member of the Standing Committee, on motion of Prof. W. B. Rogers, Lieut. Maury was appointed to fill his place for the remainder of the session.

On motion, *Resolved*, That the Secretary of the Association be authorized to procure separate books for the minutes of the General Association, and for those of the separate Sections.

On motion of Solomon W. Roberts, Esq., it was—

Resolved, That the Standing Committee be requested to memo-

rialize the Legislature of Pennsylvania, in favour of the publication by the State, of the final report of the geological survey of Pennsylvania.

On motion of Lieut. MAURY, it was—

Resolved, That the Committee on the sediment of the Mississippi river be requested to continue their investigations, with a view of ascertaining and reporting the probable effect which the reclaiming of the drowned lands of that river would have upon the improvement of its navigation, and the health of the country in the vicinity of the drowned lands.

Prof. AGASSIZ read a communication—

ON THE ORIGIN OF THE ACTUAL OUTLINES OF LAKE SUPERIOR.

He stated, that along the entire northern shore of the lake, and for some distance inland, as well as in the islands on that side of the lake, six distinct systems of dykes are exhibited, each consisting of numerous broad parallel beds of igneous erupted matter, and that the direction of the islands and successive parts of the coast line, were seen to conform themselves to the bearing of these dykes, as presented in each part of the coast. Thus it appeared that the physical outline of the shore of the lake, was determined by the geological structure of the adjoining land. He adverted to the enormous magnitude, as well as the great number of these great igneous beds, and to the vast extent to which metamorphic phenomena are witnessed in the structures and other sedimentary matter lying adjacent to the igneous masses.

In relation to the subject of Prof. AGASSIZ's communication—

Prof. W. B. ROGERS remarked, that from his observations on the southern shore of Lake Superior, and from those of other observers in that region, it was apparent that the east and west dykes, lying adjacent to that margin of the lake, had in like manner determined the chief peculiarities in the configuration of the coast. He also called attention to the fact, of the remarkable correspondence between the igneous and metamorphic masses, described by Professor AGASSIZ as existing on the northern side of Lake Superior, and the great belt of the Blue Ridge in Virginia, extending from the Potomac to some distance south of the James river. The immense extent of epidotic trap, and other masses abounding in epidote, and the various mineral

masses containing this mineral along with feldspar, as well as the infusion of epidote into the adjoining sandstones, which are presented in different stages of igneous alteration, form a feature of striking analogy between the Lake Superior district, and the Blue Ridge of Virginia. The analogy between the rocky masses, is augmented by the fact, that the epidotic rocks of the Blue Ridge, like those of the northern shore of the lake, abound in some localities in thin fibres and threads, and grains of metallic copper. Professor R. farther remarked upon the analogous geological position of the igneous and metamorphic rocks in the two cases, by stating, that the oldest of the Appallachian rocks, which lie on the west flank of the Blue Ridge, are also found contiguous to the igneous rocks on the northern shore of Lake Superior.

Peter A. Browne, Esq., followed with a communication on the subject of *Meteorites*, of which the following abstract will make known the most important points:—

Mr. Browne observed that there were fourteen theories of the origin and formation of these meteors; seven of these suppose that the materials of which they are composed, belong to either this earth or its atmosphere; in six of these seven, they are said to be generated in the air, and in the seventh, to be thrown therein from the earth. Mr. B. here described the first six theories, and then proceeded as follows: If solid meteors are generated in our atmosphere, ought not the *materials* of which they are composed to be found therein? The ærolites contain always iron, nickel, and silex; often magnesia, lime, sulphur and manganese; and rarely a trace of chrome, cobalt, tin, copper, potash, or soda; but neither of these forms a component part of our atmosphere, nor has any one been detected therein, except in these meteors. Mr. B. here enumerated the constituents of the air, and after showing the discrepancy between them, and the materials of solid meteors, he went on to show that they could not exist floating in the air. Native iron (says Mr. B.) weighs more than five times as much as water, and the air is 282 times lighter than water, —how then can native iron be suspended in the atmosphere? Professor Soldani says, that the materials have risen from our earth, but how is it possible that a substance could have risen through a medium more than 4000 lighter than itself? To get rid of this difficulty, it is supposed that the materials are first transformed to a gas; but it requires 800° of F. to raise iron to even a red heat, and 150° of

Wedgewood's pyrometer, to make a white heat, then whence comes the heat necessary to reduce it to a gas? The heat of a tropical sun imparts to iron only 200° of F. But suppose these materials, by some unknown process, to be transformed into a gas and raised in the air, would they not lose all their caloric long before they arrived at the height at which these meteors have been seen. At the height of only 1000 feet this gas would have passed $\frac{1}{13}$ of the atmosphere, at 10,600 feet it would have passed $\frac{1}{3}$ of the atmosphere, and at 28 miles, the height at which the Ohio meteor was seen, it would have passed $\frac{255}{256}$th of the atmosphere!

If these meteors are formed of materials derived from our earth, how comes it that they always contain native iron and native nickel, not found together therein.

These meteors are sometimes of great size, the one seen in Ohio was estimated at $\frac{1}{4}$ of a mile; now, if this meteor was formed in the air from materials derived from this earth, how came it to be suspended in the air until it attained this immense size, rather than have fallen to the earth in small particles like rain, hail, and snow?

If solid meteors are formed in our atmosphere, there is no rational way of accounting for their inertia force, nor for their horizontal direction as regards this planet. Nor can their great velocity be ascribed to any known or reasonable cause. And lastly, if they had emanated from this earth, they ought, according to the law of compensations, to have returned to the earth again, and not to have been seen travelling through space in every direction.

Mr. B. next proceeded to show that these meteors could not have proceeded from the moon.

In regard to the opinion of Mr. Ferguson, that solid meteors are portions of a disrupted planet, Mr. Browne remarked, that there was an objection, which was conclusive, viz. that no particles of the planet which is supposed to have exploded, could have come in sight of this earth, except those that had originally that course and direction; and then as the projective forces were equal, and they had to travel over equal spaces, they ought to have arrived in sight of this earth in nearly the same time; whereas, the first of these meteors noticed was in the 78th Olympiad, and if the disruption of the planet had then taken place, how are we to account for the retention of other fragments until the present year.

Mr. Browne, in conclusion, suggested that these meteors *might possibly* emanate from the SUN; and if that were the case, it was

probable that the planets of our system are composed of the same minerological materials.

The next communication was presented by Prof. JOSEPH HENRY, on the—

ORGANIZATION OF THE SMITHSONIAN INSTITUTION.

Professor HENRY presented to the Association an account of the organization and progress of the Smithsonian Institution. He stated, in the commencement of his remarks, that he hoped the communication he was about to make, would not be considered irrelevant to the objects of the Association, since the plan which had been adopted for the organization of the Institution contemplated co-operation with the Historical, Literary, and Scientific Societies of our country.

JAMES SMITHSON, of England, left his property, upwards of $500,000, in trust to the United States of America, in his own words, "*to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men.*"

The trust, it is well known, was accepted by our Government—the money was paid to an agent appointed to receive it, and by him deposited in the United States Treasury, in British gold. The Government therefore became responsible for the faithful discharge of the obligation incurred, viz. that of carrying out the intentions of the donor. After a delay of eight years, an act of Congress was passed August 10, 1846, constituting the President, and the other principal Executive officers of the General Government, the Chief Justice of the Supreme Court, the Mayor of Washington, and such other persons as they might elect honorary members, an establishment under the name of the Smithsonian Institution, for the increase and diffusion of knowledge among men. The members and honorary members of this establishment are to hold stated and special meetings for the supervision of the affairs of the Institution, and for the advice and instruction of a Board of Regents, to whom the financial and other affairs are entrusted.

The Board of Regents consists of three members, ex officio, to the establishment, namely, the Vice President of the United States, the Chief Justice of the Supreme Court, and the Mayor of Washington, together with twelve other members, three of whom are appointed by the Senate from its own body, three by the House of Representatives

from its members, and six citizens at large, appointed by a joint resolution of both Houses. To this Board is given the power of electing a Secretary and other officers, for conducting the active operations of the Institution.

Much diversity of opinion existed as to the plan of organization.

The act of Congress establishing the Institution, directed, as a part of the plan, the formation of a library, a museum, and a gallery of arts, together with provisions for physical research and popular lectures, leaving to the Regents the power of adopting such other parts of the organization as they might deem best suited for the promotion of the purposes of the testator.

After much deliberation, the Regents resolved to divide the annual income, thirty thousand nine hundred and fifty dollars, into two equal parts: one part to be devoted to the increase and diffusion of knowledge by means of original research and publications; the other half of the income to be applied, in accordance with the requirements of the act of Congress, to the gradual formation of a library, a museum, and a gallery of art.

They were led to this distribution and the adoption of the annexed programme by the following considerations, principally deduced from the will of Smithson:—

1. The bequest is for the benefit of mankind. The Government of the United States is merely a trustee to carry out the design of the testator.

2. The Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name.

3. The objects of the Institution are—first, to increase, and second, to diffuse knowledge among men.

4. These two objects should not be confounded with one another. The first is to increase the existing stock of knowledge by the addition of new truths; and the second to disseminate knowledge thus increased among men.

5. The will makes no restriction in favour of any particular kind of knowledge; hence all branches are entitled to a share of attention.

6. Knowledge can be increased by different methods of facilitating and promoting the discovery of new truths, and can be most efficiently diffused among men by means of the press.

7. To effect the greatest amount of good, the organization should be such as to enable the Institution to produce results in the way of

increasing and diffusing knowledge which cannot be produced by the existing institutions in our country.

8. The organization should also be such as can be adopted provisionally, can be easily reduced to practice, receive modifications, or be abandoned, in whole or in part, without a sacrifice of the funds.

9. In order to make up for the loss of time occasioned by the delay of eight years in establishing the Institution, a considerable portion of the interest which has accrued should be added to the principal.

10. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should therefore be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently supported by the Institution.

11. The plan and dimensions of the building should be determined by the plan of the organization, and not the converse.

12. It should be recollected that mankind in general are to be benefited by the bequest, and that, therefore, all unnecessary expenditure on local objects would be a perversion of the trust.

13. Besides the foregoing considerations, deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution: namely, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

The following are the details of the two parts of the general plan of organization provisionally adopted at the meeting of the Regents, December 8th, 1847, and is now in the process of being carried into execution:

DETAILS OF THE FIRST PART OF THE PLAN.

I. *To increase Knowledge, it is proposed to stimulate research by offering rewards, consisting of money, medals, etc., for original memoirs on all subjects of investigation.*

1. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled Smithsonian Contributions to Knowledge.

2. No memoir, on subjects of physical science, to be accepted for publication, which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

3. Each memoir presented to the Institution, to be submitted for examination to a commission of persons of reputation for learning in the branch to which the memoir pertains; and to be accepted for publication only in case the report of this commission is favourable.

4. The commission to be chosen by the officers of the Institution, and the name of the author, as far as practicable, conceded, unless a favourable decision be made.

5. The volumes of the Memoirs to be exchanged for the transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries in this country. One part of the remaining copies may be offered for sale, and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

6. An abstract, or popular account of the contents of these Memoirs to be given to the public through the Annual Report of the Regents to Congress.

II. *To increase Knowledge, it is also proposed to appropriate a portion of the income, annually, to special objects of research, under the direction of suitable persons.*

1. The objects and the amount appropriated to be recommended by counsellors of the Institution.

2. Appropriations in different years to be made to different objects, so that, in course of time, each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published, with the Memoirs before mentioned, in the volumes of the Smithsonian Contributions to Knowledge.

4. Examples of objects for which appropriations may be made:

(1.) System of extending meteorological observations for solving the problem of American storms.

(2.) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a Physical Atlas of the United States.

(3.) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of articles of science, accumulated in the offices of government.

(4.) Institution of statistical inquiries with reference to physical, moral, and political subjects.

(5.) Historical researches and accurate surveys of places celebrated in American history.

(6.) Ethnological researches, particularly with reference to the different races of men in North America; also, explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

I. *To diffuse Knowledge, it is proposed to publish a Series of Reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of Knowledge not strictly professional.*

1. Some of these reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labours, and to be named on the title page of the report.

4. The reports to be published in separate parts, so that persons interested in a particular branch, can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.

The following are some of the subjects which may be embraced in the reports.

I. PHYSICAL CLASS.

1. Physics, including astronomy, natural philosophy, chemistry, and meteorology.
2. Natural history, including botany, zoology, geology, &c.
3. Agriculture.
4. Application of science to arts.

II. MORAL AND POLITICAL CLASS.

5. Ethnology, including particular history, comparative philology, antiquities, &c.
6. Statistics and political economy.

7. Mental and moral philosophy.
8. A survey of the political events of the world, penal reform, &c.

III. LITERATURE AND THE FINE ARTS.

9. Modern literature.
10. The fine arts, and their application to the useful arts.
11. Bibliography.
12. Obituary notices of distinguished individuals.

II. *To diffuse Knowledge it is proposed to publish, occasionally, separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs translated from foreign languages, or of articles prepared under the direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should, in all cases, be submitted to a commission of competent judges, previous to their publication.

DETAILS OF THE SECOND PART OF THE PLAN OF ORGANIZATION.

This part contemplates the formation of a library, a museum, and a gallery of art.

1. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned sciences in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

2. The Institution should make special collections, particularly of objects to verify its own publications. Also, a collection of instruments for research in all branches of experimental science.

3. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books, first purchased, may be such as are not to be found in the United States.

4. Also, catalogues of memoirs and of books in foreign libraries, and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

5. It is believed that the collection in natural history will increase by donation as rapidly as the income of the Institution can make provision for their reception, and, therefore, it will seldom be necessary to purchase any articles of this kind.

6. Attempts should be made to procure for the gallery of arts casts of the most celebrated articles of ancient and modern sculpture.

7. The arts may be encouraged by providing a room, free of expense, for the exhibition of the objects of the Art Union and other similar societies.

8. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

9. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one, to act as Librarian.

10. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art; distinguished individuals should also be invited to give lectures on subjects of general interest.

11. When the building is completed, and when, in accordance with the act of Congress, the charge of the National Museum is given to the Smithsonian Institution, other assistants will be required.

Prof. HENRY next proceeded to give an account of the various operations in progress for carrying into effect the principles of the foregoing programme.

A number of original memoirs have been accepted for publication; the first volume of the contributions is now in the hands of the binder, and will be ready for delivery to public institutions in the course of a few days. It consists of a single memoir on the antiquities of the Mississippi valley, by Messrs. SQUIER and DAVIS. The manuscript was submitted to the American Ethnological Society, and had been pronounced by a committee, appointed to examine it, a highly interesting addition to knowlege, resting on original research, and fully worthy of a place in the Smithsonian contributions. In order to remunerate the authors for the expense which occurred in the investigations detailed in this work, the Institution allowed them to strike from the same plates and type an edition which they might sell for their own benefit. No expense has been spared on the mechanical part of the volume—the best artists have been employed, and the execution will bear comparison with any book of the kind yet published in this country or Europe. Preparations have also been made to commence the second volume, which will consist principally of a series of original miscellaneous papers on different branches of physical science.

The Librarian has been engaged in collecting statistics of libraries, and in forming collections of catalogues and other works, required in

carrying out the plan of rendering the Smithsonian a centre of bibliographical knowledge, where the student may be directed to any work which he may require.

In order to facilitate the study of American History, the Institution has agreed to publish and distribute to our public institutions, a work comprising a description of all books relating to America, prior to the year 1700, which may be found in the public and private libraries of Europe and America, or which are described in other works, together with notices of many of the more important unpublished manuscripts. The names of the different libraries, both in this country and in Europe, in which the books are to be found, will be mentioned.

The expense of preparation of this work will be defrayed by subscription, and such is the estimate of its importance, as well as the confidence reposed in the gentleman (Mr. Henry Stevens) who has engaged in this enterprise, that subscriptions to the amount of $5000 have already been made by several public institutions and liberal individuals. Mr. S. has just sailed for Europe to commence his labours, and intends to employ a number of assistants in England, and in different parts of the continent.

Under the head of Original Researches, preparations are making for the establishment of an extended system of meteorological observations, embracing, as far as possible, the whole American continent. It is believed that the co-operation of the British government will be obtained, as well as that of several States of our Union. Also, a series of explorations have been established with reference to terrestrial magnetism, and other subjects connected with the physical geography of the United States.

It is intended to introduce, gradually and cautiously, the other parts of the plan; and, indeed, according to the present arrangement, the programme could not be carried into full operation until after three years from next March. Up to that time one-half of the whole income is to be devoted to the building.

The professor stated, that almost every day furnished him with new evidence of the importance to the science and literature of our country, which might be made to result from a judicious expenditure of the income of the Institution. He regretted that in order to make provision for the accommodation of the Museum of the exploring expedition, as directed by the Act of Congress, so large an amount of money was required for the erection of the buildings. The evil, however,

which would result from this is, in a measure, obviated by the plan proposed by Prof. BACHE, and adopted by the Regents, viz., that of deferring the time of completing the building, so that it might be erected, in considerable part, by means of the interest of the $240,000 which had accrued in interest on the original fund previous to the year 1846. By a rigid adherence to this plan, it is calculated that at the end of a year from next March, after paying for the building, $150,000 will be added to the original fund, making the whole $650,000.

In conclusion, Prof. H. had much pleasure in announcing to the Association, a munificent gift to science, by one of our members, Dr. HARE, of Philadelphia. This gentleman, having resigned the chair of chemistry, in the medical department of the University of Pennsylvania, has presented to the Smithsonian Institution the instruments of illustration and research, collected and used by himself during his long and successful career. The gift is of high importance, not only on account of the value of the instruments, many of which are the invention of the donor, and connected with the history of the science of our day, but also on account of the example of liberality which it affords, and which, we trust, will be frequently followed by others.

Prof. W. B. ROGERS, from the Standing Committee, mentioned that P. A. BROWNE, Esq., from the same Committee, would report the resolution as modified by that body, relative to the death of Professor LARDNER VANUXEM, which resolution had been unanimously adopted by the Committee, and hoped the same would be as unanimously concurred in by the Convention.

Whereupon Mr. BROWNE reported the following, prefaced by a preamble of his own, relating to the character and eminent qualities of the deceased:—

"*Resolved*, That the Association learns with deep regret and sorrow, the death of Professor LARDNER VANUXEM, a member of the Association, and one of its most active founders; that Prof. HALL be requested to draw up an obituary to his memory, and that a committee of three be appointed to address a letter of condolence to his family and friends, as the expression of sympathy of this Convention."

In advocating this resolution, Mr. BROWNE remarked, that the late lamented Professor LARDNER VANUXEM was educated at the School of Mines, in France, and that his whole life was devoted to science.

He belonged, therefore, to his whole country, but he had been particularly identified with three great States; 1st, Pennsylvania, who gave him birth; 2d, North Carolina, where he made the first State Geological Survey; and, 3d, New York, where he was one of a small band who had produced a geological survey, the maps and descriptions of which had commanded the admiration of the world.

Mr. BROWNE would also add, that Mr. VANUXEM and Col. Long were associated with him in making to the Legislature of Pennsylvania the first proposition of a geological survey of this State.

After some impressive and eloquent remarks on the character of the deceased, by Dr. R. W. GIBBES—

The following letter from Prof. HALL, in relation to the character and services of Mr. VANUXEM, was also received, read, and ordered to be entered, at length, in the proceedings of the Association.

Philadelphia, Sept. 21, 1848.

To the Chairman of the American
Association for the Advancement of Science.

Sir,—I observe, by the published proceedings of yesterday, before my arrival in the city, that the attention of the Association was called to the fact of the decease of one of our members (LARDNER VANUXEM) during the last year. Had I been present, I would have offered some remarks as a tribute to the character and worth of a friend and colleague, whom it has been my happiness to know for twelve years. A friend, whose kind acts were never ostentatious, and who was willing to be forgotten while doing good and aiding the cause of science.

At this time, as we are met for the first time, under our new organization, I cannot forbear recurring to the origin of our Association, with which Mr. VANUXEM was so intimately connected, and to whom is due, above all others, the honour of being the first man to propose such an organization.

The geological survey of New York was commenced in 1836. At the end of each season of field labour, and on one or two occasions previous to the commencement of our summer labours, all those engaged in the work met, by arrangement, at Albany, for the purpose of comparing observations and acquiring knowledge of each other. At the end of the second year, the want of some geological nomenclature began to be seriously felt, more particularly as we found that the same rock was well developed in two or more districts of the State,

and consequently some language must be devised to enable us to understand each other. We had, moreover, no means to make ourselves understood among our friends in other parts of the country. In the autumn of 1838, Mr. Vanuxem proposed that an invitation be given to the geologists of Pennsylvania and Virginia particularly, to join us, and that this invitation be extended to the geologists of other States, to meet at Philadelphia for the purpose of devising and adopting a Geological Nomenclature which might be acceptable to all those then engaged in the State Surveys, and thus become the nomenclature of American Geology. The first meeting was to have taken place in the spring of 1839, but from want of concurrence among those widely separated from each other, it was not accomplished till 1840. This meeting, having for its object the expression of our views and the adoption of a general Geological Nomenclature, was the first meeting of what we then termed the Association of American Geologists. That meeting, and the subsequent one, of 1841, held also at Philadelphia, the subjects discussed were all of a geological character. The first objects of this Association were not accomplished at that time, for the reason that, in some States, the investigations were still in progress, and regarded as too incomplete to found a nomenclature. Thus the subject was finally dropped, since, in 1842 and 1843, the geologists of New York were compelled to publish their reports, adopting a local nomenclature till a better can be framed.

The special science for the advancement of which this Association was formed, has been merged in more important considerations, and is now almost forgotten. The Association now embraces in its objects, the investigations in all the physical sciences; but while we are thus enlarging our boundaries, and increasing our numbers, we may not forget our humble beginning, or the man who was the instrument in bringing together, in this reunion, his fellow labourers in science.

I have the honour to be,

With great respect,

Your obedient sèrvant,

JAMES HALL.

The resolution, as amended, was unanimously adopted.

The Standing Committee reported, through its Chairman, the nomination of a list of officers for the ensuing year, as follows:—

For President—Prof. Joseph Henry, of Washington.

„ Secretary—Dr. Jeffries Wyman, of Boston.

„ Treasurer—Dr. A. L. Elwyn, of Philadelphia.

Local Committee—Messrs. NATHAN APPLETON, J. A. LOWELL, JACOB BIGELOW, M. D., GEORGE B. EMERSON, Prof. H. D. ROGERS, A. A. GOULD, M. D., ENOCH HALE, M. D., Boston; Lieut. C. H. DAVIS, ASA GRAY, M. D., Prof. BENJAMIN PEIRCE, Prof. E. N. HORSFORD and Prof. LEWIS AGASSIZ, Cambridge.

These nominations were, on motion, unanimously confirmed.

The Chairman of the Standing Committee reported the following resolution, which was, on motion of Lieut. MAURY, unanimously adopted:—

Resolved, That the committee on the sediment of the Mississippi river be requested to continue their investigations, with the view of ascertaining and reporting the probable effect which the reclaiming of the drowned lands of that river would have upon the improvement of its navigation, and the health of the country in the vicinity of the drowned lands.

On motion, adjourned, to meet this evening at half-past 7 o'clock.

Saturday Evening, September 23, 7½ P. M.

Association met agreeably to adjournment—W. C. REDFIELD, Esq., in the chair.

Prof. W. R. JOHNSON presented a communication—

ON SOME RECENT IMPROVEMENTS IN RECORDING MAGNETIC TELEGRAPHS.

He first exhibited apparatus, illustrative of the difference between indicating and recording telegraphs, and explained the several ways in which it had been found practicable to produce permanent impressions by galvanic agency. The plan of Morse, which employs the force of temporary magnets to produce indentations on paper, involves the necessity of using, at each telegraphic station, a local battery, solely devoted to the working of the marking apparatus. This local battery is brought into action by the current of the main line, which had been found too feeble to effect the mechanical action necessary for impressing the paper. The use of a local battery is both expensive and inconvenient, and many efforts have been made to dispense with its employment, while attaining all the advantages of the system of permanent marking. The means of accomplishing this have been attained by Mr. Winegar, a telegraph operator, of Cayuga county, New York, who, instead of marking with a pen set in motion by the current of the main line, as first attempted by Mr. Morse, or of im-

pressing the paper by the aid of the local battery, as now practised by the same inventor, employs the main line current to give a slight oscillatory motion to the paper, bringing it alternately into and out of contact with the point of a stationary pen. His invention embraces also, a mechanical arrangement for supplying the pen with ink, according to the exigencies of the work, and another arrangement for adjusting the position and movement of the paper. His claim extends to the use of the main telegraphic current for the purpose of giving motion to the paper, on which the marking is to be performed.

Prof. HENRY alluded to the improvement of Mr. Bain, lately introduced into this country, as affording another means of working a recording telegraph, but remarked, that as the invention had not yet been made public here, he could only say that he regarded it as a highly ingenious and beautiful application of science to the purpose for which it was employed.

Prof. AGASSIZ next presented a communication "ON THE FOSSIL CETACEA OF SOUTH CAROLINA." [Not received.]

Prof. WILLIAM B. ROGERS followed with a paper—

ON ACID AND ALKALINE SPRINGS.

In this communication, after referring to the principal classes of mineral springs, thermal and of ordinary temperature, and comprehended under the terms acidulous, saline, sulphuretted and chalybeate, Prof. R. entered into a particular account, geological and chemical, of two very distinct classes of springs of very frequent occurrence in the Appallachian region, particularly in Virginia and Eastern Tennessee. The one is remarkable for containing a considerable amount of *free sulphuric acid*, along with sulphates of iron and alumina, the other is distinguished by containing a small quantity of *carbonate of soda*, along with carbonate of lime and magnesia, much silica and some carbonic acid and sulphuretted hydrogen.

These springs are of very common occurrence in the slates and shales, known as the Marcellus, Hamilton, &c., in the New York series, and designated in the nomenclature of the Profs. Rogers, as the post meridial, older and newer slates and shales respectively. In those parts of these formations, which abound greatly in decomposing bisulphuret of iron, and which are not interstratified with calcareous beds, the springs which occur belong to the former of the two classes. Such, for example, are the celebrated Alum springs and Brinkley's

springs near the eastern base of the great Warm Spring mountain in Virginia. At these and similar localities, the crumbling slates are imbued with the products of the decomposed pyrites, and yield to the infiltering waters a portion of free acid, as well as sulphates of iron and alumina. But what is specially remarkable in the composition of these waters, is the fact, that the proportion of free sulphuric acid present, often very greatly exceeds that which the oxidation of the bisulphuret could furnish. This excess Prof. R. proposed to explain in the following way:—While the bisulphuret is subject to oxidation, as above mentioned, a part of the sulphates thus formed, reacting with the organic matter, always present in these rocks, gives rise to sulphuretted hydrogen gas. This again, as recently shown by Dumas, in the presence of air and organic matter, gives birth to sulphuric acid—and thus the additional supply of this acid, formed at the expense of the sulphates, will be imparted to the percolating water.

Of the second, or alkaline springs, Prof. R. stated, that they were found in the same general slaty belt with the others, but always in connexion with those parts which contain more or less *carbonate of lime*. Instances of these springs are seen in the Grey Sulphur and Dibbrell's springs, as well as many others in Virginia and Tennessee.

The absence of sulphuric acid in these waters, is an obvious consequence of the reaction between the carbonate of lime and the acid in passing into the mass. The same reaction giving rise to the evolution of a great amount of carbonic acid, would, as it were, saturate the pores of the slate with this substance, which, in virtue of its large excess, would have power to decompose the sulphuret of sodium, and perhaps other salts present, and thus give origin to the small amount of carbonate of soda, which imparts alkalinity to these waters. The great proportion of silica, in the solid residuum of these springs, may doubtless be ascribed to the solvent power of the alkaline carbonate.

Professors W. B. and R. E. ROGERS read a paper—

ON THE COMPARATIVE SOLUBILITY OF THE CARBONATE OF LIME AND THE CARBONATE OF MAGNESIA.

In presenting these results, reference was made to the statement in the standard chemical and geological works of the much greater solubility of carbonate of lime than carbonate of magnesia. This assumed fact has been made the basis of a theory of the origin of the large pro-

portion of carbonate of magnesia in the magnesian limestones. It has been supposed that, in a mixed limestone, containing both the carbonates, the relative amount of carbonate of magnesia would be augmented by the action of the percolating water, in consequence of the more rapid removal of the carbonate of lime, and in this way it has been proposed to explain the curious fact of dolomization.

The experiments of the Profs. R., prove that, in water impregnated with CO_2, *carbonate of magnesia is more soluble than carbonate of lime.* Thus, by allowing the slightly carbonated water used in these investigations to filter through a mass of finely powdered magnesian limestone, and collecting the clear liquid, it was found to contain a much larger proportion of carbonate of magnesia in comparison with the carbonate of lime, than corresponded with the amount of these substances relatively in the powdered rock. Again, by agitating briskly a quantity of the powder with the carbonated water in a glass vessel, and then separating the liquid by filtration, it was observed that a larger relative amount of the carbonate of magnesia had been taken up, than of carbonate of lime.

From these experiments, it is inferred that the infiltering rain water, with its slight charge of carbonic acid, in passing through or between strata of magnesian limestone, will remove the carbonate of magnesia more rapidly than the carbonate of lime, and that thus the rock will gradually become relatively less magnesian, instead of being made to approach the condition of a dolomite, as has been maintained.

Profs. R. called attention to the fact, that the stalactites in caverns of magnesian limestone contain only minute quantities of carbonate of magnesia. An examination of those in Weyer's cave, in Virginia, had proved that only the milky white opaque stalactites contain a marked amount, and that the sparry transparent kinds are almost destitute of magnesia. It would thus appear that the carbonate of magnesia, in virtue of greater solubility, is carried off by the liquid from which the carbonate of lime is in part deposited.

Prof. W. B. Rogers appended some remarks upon the connexion between the mode of stratification of the impending rocks, and some conspicuous features of the stalactitic masses. In caves formed in horizontal limestone, without numerous cross fissures, the stalactites are comparatively infrequent and of small extent. Such is the case in the great Mammoth cave of Kentucky. In those which are placed in axes, or in the midst of steeply dipping strata, as is the case with Weyer's cave, the infiltering waters finding innumerable points of access in the roof along the lines of bedding, give rise to an abundant

accretion of stalactites which depend every where from the roof, and clothe the walls in rocky drapery. In the cave in question, *vast sheets of stalactite* are seen stretching from the roof to the floor. These thin parallel plates have the same direction with the divisional planes of the overhanging beds of limestone. Profs. R. traced their formation to the production, in the first place, of a line of stalactites, along the seam or division in the roof. By farther accretion from the water trickling over their surfaces, these would be made to coalesce, and thus the shape of the whole would at length become that of a sheet, or thin mass gradually extending to the floor.

After the conclusion of this paper, the Association adjourned till Monday morning next at 9 o'clock.

WALTER R. JOHNSON, *Sec'ry.*

Monday morning, September 25, 9 o'clock.

The Association met agreeably to adjournment, Wm. C. Redfield, Esq., President, in the chair.

The first business in order was a report of business by the Standing Committee, who, through their chairman, proposed:—

1st. That Asa Whitney, Esq., be admitted to membership in the Association, which was adopted.

2d. That Lieut. M. F. Maury be added to the Committee on the Sediment of the Mississippi river, which was likewise adopted.

3d. That an abstract of each communication submitted to the Association during its sitting, be furnished, by its author, for publication in the proceedings. Adopted.

4th. That a committee of ten be appointed to memorialize the President of the United States, the Heads of Department, and Congress, upon the promotion of science, by adding to each public expedition which may be organized, a number of scientific men, whose observations may tend to this end.

On this last proposition, remarks were made by Dr. Hare, Prof. Agassiz, and others.

Professor Hare observed, that our government was more ardent after military renown, than the great mysteries of scientific truth, and that any aid from that source would be extremely difficult to secure. The object of power, it would seem, was to imitate the silken non-

sense of courteous Europe; and that if gentlemen in station could acquire a knowledge of this, their great aim was accomplished. Our central government was extremely remiss, and had, instead of giving to our people an example, upon all occasions, of dependence upon self, its greatest pride was to copy from the stated and almost obsolete theories of the old world, all that pertained to government, as well as that which related to national habits and the advance of science. It was, what he styled, borrowing a mirror to reflect back our own character; and he hoped the day had arrived when this practice would be put an end to. If in nothing else, let us at least be original in our scientific structures. We have the material, and it should be our pride to build up our own academies. But we were too much wedded to foreign flattery, he feared, to effect this purpose without a struggle. If France, for instance, passed upon us her notice—that is, by faint praise damning us—we were intoxicated in a moment, and all our promises to rely upon ourselves, levelled without exception. He admitted that it was necessary to keep up a Navy and a "Standing Army," but he thought that in peace, if the officers must needs receive an academy education, that they should not be encouraged in idleness. Let them be usefully employed. West Point turned out yearly a number of well taught, scientific men; and for what purpose? To guide a rudder, or point a gun? He hoped not. Let these, then, be detailed for the service mentioned in the resolution. What else was their education fit for? He trusted that the committee would be importunate in their memorial, and not rely upon the effect of a mere formal application.

Professor AGASSIZ was under the impression that we should rather depend upon our own energies and exertions, than the patronage of government. Science, from time immemorial, was an object of neglect, and if in other countries, where power revelled in a different element, it should be forgotten, why should it not be here? Men of science, to succeed in their operations, must, on the contrary, be entirely unknown to what we call the "world." Patronage of government is not worth the time consumed in imploring it. We must stand alone, and while enduring neglect, labour the harder. All the governments upon earth will not, in one year, develop as much scientific knowledge, as one humble individual, whose lot is cast in a garret.

After some further discussion, the proposition was adopted, and the following gentlemen were appointed by the Association to constitute the committee on said memorial:—

Dr. ROBERT HARE, of Philadelphia; Prof. BENJAMIN SILLIMAN,

Senior, of New Haven; WILLIAM C. REDFIELD, Esq., of New York; Prof. BENJAMIN PEIRCE, of Cambridge, Mass.; Prof. STEPHEN ALEXANDER, of Princeton; Dr. ROBERT W. GIBBES, of Columbia, S. C.; Prof. HENRY D. ROGERS, of Boston; Prest. EDWARD HITCHCOCK, of Amherst, Mass.; Prof. LOUIS AGASSIZ, of Cambridge; Dr. SAMUEL G. MORTON, of Philadelphia.

The Secretaries of the Sections of General Physics and of Natural History, severally, made reports of the doings of those Sections on Friday and Saturday last.

Dr. R. W. GIBBES, from the Natural History Section, reported the following papers, read and discussed before that Section on Friday last:—

On the Forces which have caused the Rupture, Contortion, Depression, and Upheaval of the Superficial Strata of the Earth, by Prof. L. J. GERMAIN.

On Phacops Hausmani, by Prof. S. S. HALDEMAN.

Report on the Sediment of the Mississippi river, by Dr. M. W. DICKESON.

Also the following, read on Saturday, 23d inst.:—

On Terraces and Ancient River Bars, the Drift, Boulders, and the Polished Surfaces around Lake Superior and Lake Huron, by Prof. AGASSIZ.

On the Black Banded Cyprinidæ, by Prof. AGASSIZ.

On the Cyprinodonts, by Prof. AGASSIZ.

On Acid Springs and Gypsum Deposits of the Onondaga Salt Group, by Mr. T. S. HUNT.

On Rhamnus Lanciolatus, by Dr. T. GREEN.

On the Geography and Geology of the Northern Mississippi, by R. BOLTON.

Prof. SILLIMAN, jr.'s, Report for Friday and Saturday morning was not received.

The following communication was received from the Society for the Development of the Mineral Resources of the United States:—

Philadelphia, June 10, 1848.

To the American Association
for the Advancement of Science.

It becomes our duty, as it is our pleasure, to announce the formation of a Society, in this city, which has been denominated, "*The Society for the Development of the Mineral Resources of the United*

States." With unabated admiration for all that part of geology which is *strictly* scientific, we devote ourselves *particularly* to its *œconomical* department.

We propose to collect and expose, gratis, to public view, specimens of all the rocks and minerals of the Union which are useful in architecture, agriculture, manufactures, or the mechanic arts, with a view to make them more generally known, and to increase their consumption in this our much favoured land. We desire to open and maintain, with your Institution, a frank and free correspondence, to exchange sentiments upon all subjects connected with our pursuits, and to give and receive, upon the most liberal terms, duplicate geological and minerological specimens, and, generally, to co-operate with you in all measures that may tend to increase the mineral wealth and consequent prosperity of our beloved country, or to elevate the mental character of its inhabitants.

With our best wishes for your advancement, we have the honour to be

Your obedient servants,

PETER A. BROWNE, *President.*

M. W. Dickeson, *Cor. Sec'ry.*

The invitation given in the above letter, was, on motion, accepted.

The following communication, from Mr. Desor, relative to a deposit of drift shells in the cliffs of Sancati Island, of Nantucket, was read, and ordered to be entered in the proceedings:—

W. C. Redfield, Esq., President of the
American Scientific Association in Philadelphia.

Dear Sir:—In visiting last summer, in company with Lieutenant Davis, U. S. N., the neighbouring islands and shoals of Nantucket, we discovered a most remarkable deposite of Drift Shells in the cliffs of Sancati. These cliffs, which form the eastern boundary of the island of Nantucket, are nearly 100 feet high. The shells are found in two distinct strata, at a height of 30 feet above the cliffs. One of these strata, is a regular oyster bed, 3 to 4 feet thick, in which not only the oysters, but also the venus, nya, and other bivalves are found in their natural position, both valves together, showing that the animals died in the place in which they lived, and that they have not been disturbed by any violent action, although they have been considerably raised, since there is 50 feet of sand deposite above them, as may be seen in the following section. From the annexed list of species that

have been found, both by my friend Edw. Cabot and by myself, you will perceive that they are the same species that occur near Brooklyn, and also, although in a very broken state, at Point Shirley, near Boston, and that they constitute a thorough littoral fauna, exactly similar to that of the shores of Nantucket and Long Island, actually.

The second stratum in which shells are found, rests immediately on the oyster bank, being separated from it in some places by a stratum of serpula, that belongs to the oyster bank. This second stratum, one and a half to two feet thick, is quite different in its appearance from the oyster bank below it, although the species that are found in it, are the same. The shells are all bleached, and more or less worn; the barnacles are disintegrated, and the bivalves have very seldom the two valves united, showing that they have been exposed for a certain time to the action of the waves, before they were buried by the sand layers above them. These sand deposites, like the sand, clay, and gravel strata that underlie the shell strata, are entirely destitute of fossil animals. The only organic remains I discovered, was a fragment of wood, in a white sand layer below the oyster bank. At first these strata seem to be horizontal, but Mr. Cabot has since ascertained that they all slip somewhat to the west, their inclination varying from 6° to 15°. At the top of the cliff is found a layer of fine sand, quite irregular in its outline, being formed by the winds that carry the fine particles of sand from the beach to the top of the cliff.

The basis of the cliff is totally different from all the strata mentioned. It consists in a deposite of dark sandy clay, without any distinct stratification in its interior, but its upper surface being very much inclined to the west, and therefore quite unconformable with the sand and shell strata that rest on it. Although no fossil have yet been found in this clay, I feel inclined to consider it as identical with the lower strata of Gayhead, being probable the eastern outcroping of a tertiary basin that underlies the island of Nantucket and Martha's Vineyard, and extends to the north till at Cape Cod. It will be shown in a special paper, on this subject, that this tertiary formation has been deposited in form of a bank or shoal in the ocean of the tertiary epoch, according to the laws of tidal action, that are so satisfactorily pointed out by Lieutenant Davis, the materials of this tertiary shoal being the detritus of the chalk or green sand formation that is found on the shores of the middle States of the Union.

Believe me, my dear Sir, yours, most sincerely,

E. DESOR.

Boston, 18*th Sept*. 1848.

Mr. REDFIELD remarked, in relation to the subject of the last communication, that he had found at Brooklyn and on the coast of New Jersey, similar deposites of drift shells.

Prof. AGASSIZ also submitted some observations in regard to the discoveries of M. DESOR.

Messrs. ROBERT E. and W. B. ROGERS now read a paper, containing the results of investigations on ACIDIMETRY and ALKALIMITRY. [Not received.]

Mr. STEPHEN P. ANDREWS then gave, by chart illustrations and verbal statements, the analogies between the Chinese written and spoken language.

He exhibited the signs and characters used by the Chinese, to distinguish the same words which occur in the English language, as, for instance, "wright," "right," "write," &c. The word "*ting*," in the former language, indicates the same sound, and, by an additional affix or prefix, the intelligence meant is conveyed.

Prof. JOHNSON communicated the following report, by J. H. ALEXANDER, Esq., on Geographical Explorations during the year 1847 and a part of 1848. The paper is entitled—

BRIEF NOTICE OF GEOGRAPHICAL EXPLORATIONS AND RESEARCHES DURING THE YEAR 1847 AND A PART OF 1848.

1. *Africa.*

The most important exploration actually going on in this continent, is that of Mr. ANNE RAFFENEL; both as regards the amplitude of design, the aids and connexions under which it is being prosecuted, and the probable fitness of the individual explorer himself. Prepared for the undertaking by special preliminary studies and exercises, and having already approved himself by the experience of travel in 1844, in Upper Senegambia, he takes with him now the necessary apparatus for scientific researches, instructions from the Academy of Sciences and the Geographical Society of France, and funds supplied, in part at least, by the French Government. How far the recent changes in that government will affect this last particular, and alter the semi-official aspect, which his mission wears, is not yet known. The original design is no less than to cross the entire continent, in general along the 15th parallel of N. latitude. In fact, ascending the Senegal, his course would take him into the valley of the Jolibah, by Sego and Timbuctoo (at the former of which places he most probably winter-

ed), through Central Soudan. The Quorra, in part, the Yeon, and then some eastern tributary of Lake Tchad, are supposed to point and facilitate the route through to Darfur and Kordofan; whose yet unknown territories once passed, bring the traveller to the comparatively friendly waters of Nile. Not the extent of the journey only—though from Cape Verd, for instance, to Shendy, on the Nile, is, along the parallel, nearly four thousand miles, while the actual track will be much longer—but the perils and difficulties, physical, moral and spiritual, which environ and beset every inch of the way, from Sego to the Nile, constitute the drawback to this superb design; whose execution will leave the achievements of preceding travellers, on that continent (already watered with blood as high as ever flowed in manly veins) quite in the shade, and will solve one of the great problems of North African geography.

Connected with this exploration of Soudan, may be mentioned the publication, in 1847, by Dr. Rosen, interpreter attached to the Prussian Legation at Constantinople, of a German translation from the Turkish Narrative of a Journey made about 1800, in the negro kingdom of Daz-Zaleh or Wadai (between Begharmi and Darfur, in the easternmost part of Soudan or Nigritia), by Scheich Zaïn el Abidin, a Mohammedan of Tunis. The original narrative in Arabic has not come to the hands of Dr. Rosen. It is much to be desired that some traveller in Tunis would make research to recover the text. The territory of Wadai is in Mr. Raffenel's route; but, except some vague information of Brenne, Seetzen and Burckhardt, it was before Dr. Rosen's publication quite unknown to Europeans. It is true, that there exists in MS., and in the hands of Mr. Jomard (who caused to be published, in 1845, a French translation of the part relating to Darfur), the narrative, more recent, of another Tunisian, Scheich Mohammed el Tounsi, who also travelled in Wadar. This may be expected to be published; and although, from the religion and education of both travellers, not many important additions to our knowledge are to be looked for, still it would be both piquant and instructive to read and compare these two accounts in advance of the more important and reliable one which we may hope is reserved for Mr. Raffenel to afford.

It is reported, very recently, that, under the stimulus of this publication of Dr. Rosen, the King of Prussia is about to send a scientific commission to explore the architectural remains signalized by El Abidin. It is more probable, however, that the present unsettled state of European affairs will contribute to retard this desirable measure.

In conclusion of the subject of Nigritia, may be mentioned the recent publication under the sanction of the British Colonial Office, and of the Admiralty, by Messrs. Allen and Thomson, of the Narrative of the Expedition to the Niger, in 1841, under Captain Trotter.

On the eastern coast of the continent, an exploration is going on under the auspices of the East India Company, and in charge of Mr. Parker, one of their marine, to ascend the Jub, a river of Zanguebar, falling into the Indian ocean, about the latitude of the equator. Should this prove to be identical with the Godjeb, part of whose course only is known, there would be opened an easy communication with the southern part of Abyssinia; a route of high importance in a commercial point of view, but which, although it has furnished, since a long time, plausible grounds of entertainment, was never seriously taken up until a few years ago, by the lamented missionary, Krapff. The current of recent investigations, however, it must be said, is against the identity mentioned; and Dr. Behe, one of the first to assert it, is now, upon the results of the exploring expeditions, sent by the Viceroy of Egypt, inclined to regard the Godjeb as one with the Telfi or Sobat, a confluent of the Bahr-el-Abiadh or White Nile. But it does not follow, even upon this, that the route contemplated may not be substantially obtained.

Another traveller, Mr. Leigh, has been sent, by a London company, to Quiloa, on the Zanguebar coast, more south; in order to penetrate by the Mozimba or some other stream, westward, to the great Lake N'Yassi, the Maravi or Zambezi, of our older maps. Apart from the commercial importance of success in this exploration, it has, just now, an additional scientific interest, in connexion with the opinion advanced by one of the best informed and most competent Abyssinian travellers—Dr. Behe, just mentioned—that this Lake Marair is the true head of the Nile.

The annunciation of this opinion, to be sure, has been followed by an expression of dissent, on the part of another distinguished traveller, if he is not rather to be called a resident in Abyssinia—Mr. d'Abbadie—whose grievous silence, protracted through nearly two years, had given rise to the most sorrowful forebodings. Far from allowing to the Nile such an extension as would cause its source to be found south of the equator, Mr. d'Abbadie supposes its real fountain to be that of the Gibé in Jwarya (about 7° 49′ N. lat.), whence, with a semi-circular sweep under various names of Omo and Uma and Bagno, it makes a Chersonèse of the territory of Kaffa. The developments of our ordinary maps do not allow this course to be traced with intelligible

precision; and these opposite opinions of the two travellers cannot, in the present state of our knowledge, be rested upon higher grounds than conjecture. The return of Mr. Antony d'Abbadie, which may now be looked for, will, no doubt, add vastly to our definite information upon the region generally, and may even afford the elements for a decision of this very question. In the mean time, it is unfortunate that two travellers, so eminent in their opportunities for information, and to the opinion of either of whom alone so much would justly be conceded, should be found coming to such opposite conclusions; and, still more, that they should allow themselves to introduce irrelevant personalities into a discussion, characterized, on the one side, by something bordering on flippant contemptuousness, and, on the other, by a not more agreeable though more decorous asperity.

The brothers d'Abbadie will be remembered by those who recall the account, some years ago, of the British Embassy to Shoa, as having been mixed up in the diplomacy of Combes and Tamisier, and, somehow, cognizant of the steps which led to the unceremonious ejection of the English Missionaries, Irenberg and Krapff. A sympathetic feeling on these events has, therefore, not unnaturally, created an unfavourable impression in England against the d'Abbadie; and the elder, Antony, in particular, has been charged, at any time in the last eight years, with having affected the externals of science in order to procure opportunity for carrying out certain designs in which real devotion to that goddess had no share. It is to be regretted that such charges should have been made, even if entirely without colour; they cannot be taken as proved by geographers here in America, where no such prejudice, as has been alluded to, exists; and, finally, all that need be said in this regard is, that the d'Abbadie have shown an amount of long and patient daring in apparently scientific pursuits, which has been rarely equalled; and that, if they have, in reality, all the while been but agents of the French government, in the prosecution of secret designs, worthy or unworthy, either their taste is singularly perverted, or they have allowed themselves to be used up for very small consideration.

The rumours of the friendly detention of the d'Abbadie, by some Galla chief, who could not bear to part with them, which were current from Cairo about the middle of last year, have turned out to be too favourable a version of the fact. It appears, from Mr. d'Abbadie's own narative, that in the latter part of the year 1844, having become dissatisfied upon a discussion and assemblage of his materials, with the conclusion he had hitherto taken up, as to the origin of the White

Nile, he resolved upon no less an undertaking than to retrace his steps to Jnarya for fresh observations—an operation of from four to six months. While at this last mentioned place, his brother Arnauld had become involved in the anti-European odium following the ill-reflected, as far as intelligible, achievements of Messrs. Bell and Llewden, who took active part in some of the Oalla campaigns or bush-fights. The result was, that the brothers did not meet for more than twelve months; and their position in Damot, for nearly as long afterwards, although one of armed defiance, was practically a quasi captivity. Although, however, shut out in this manner, from the principal object, their time may be understood not to have been fruitless; and the narrative of this episode alone may be expected to furnish a rich and lively repast for other than merely geographical inquirers.

A similar narrative from the unfortunate Krapff is now no longer to be looked for. Unheard from since January, 1845, when he addressed his very interesting letter upon the Ethnology of the Eastern African Coast, from Cape Guardafui southwards, there is every reason for the melancholy conviction that he has fallen a victim to the climate, to the anger or cupidity of the tribes among whom he was risking himself, or to accident, from which none are exempt. With him it will be admitted that the promotion of science was only a secondary aim; but this, though it serves to palliate our regrets, will not diminish our esteem; for the service of religion leads to a higher throne than that of human intellect, and the love of souls passes, in all regards, far beyond the love of knowledge.

In connexion with what was just now said of the Galla tribes, should be mentioned the opinion advanced by Dr. Behe, in a learned paper before the British Association for the Advancement of Science, on 5th July, 1847, touching the locality of their origin. This he places, on comparison of information by Krapff and Cooley, and oral traditions gathered by himself in the country of the Meremingäo, about two months' journey west from Mombay, and north of the Meno Moëzi, or Kingdom of the Moon, in which, as already said, he finds the sources of the Nile. Of course the Galla hypothesis blends itself, for proof, with that of the Nile; the letters of Mr. d'Abbadie content themselves with very summarily rejecting the former—the latter has been discussed very recently in a paper before the English Geographical Society, by Mr. Frederick Ayrton, a friend and correspondent of Mr. d'Abbadie.

An addition to our ethnographical knowledge of these latitudes, and further south, Zanguebar, Mozambique, and Matapa, has been

made by Mr. Eugene de Froberville in the collection of specimens and documents, as well as in a memoir to the Geographical Society of France. These researches he was enabled to make, not in the territories themselves, but during a residence of several years in the Isle of Bourbon, the colonial slaves in which are originally from the continent opposite.

Yet farther south, the territory of Natal and the country of the Caffre tribes, up to the tropic of Capricorn, have been illustrated in the publication of the six years' travels (from 1838 to 1844) of Mr. Adolphe Delegorgue; while immediately west of these districts, Mr. Lemne, a French Protestant missionary here, the first of Europeans, penetrated and introduced to our knowledge, in an excellent monograph, the country of the Kalagari, a Beehnan race, with interesting details upon the natural history of the region, and the civil and social state of its people. To enable the reader to identify the Kalagari region upon a map, it may be stated to extend between the 21st and 27th parallel of south latitude, and in longitude from 22° to 28° east of the meridian of Greenwich.

The order that has been pursued has brought us now to the comparatively civilized, and at least long-occupied Cape Colony—the southernmost termination of the African continent. And if, under this head, the Journal of Mr. Banbury, and the notes of his excursions into the interior, with its accessories of natural history and ethnology, should still be too miscellaneous to admit of being classed strictly as a scientific contribution, and the results of Sir John Herschell's observations during 1834—1838, should be claimed as a peculiar monument for astronomy, the former must nevertheless be referred to by all who wish to post up their information upon this important locality in Indo-European communication, and the latter will justly be considered as a precious addition to the treasury of mathematical geography.

From this point, the Cape of Good Hope, the steps of the continental traveller must be turned northward, to trace an unfrequented and inhospitable country and climate—enough to deter the most adventurous. Except a letter from Mr. de Saillet, upon the coast of Cimbebas, there is no illustration of the great stretch from the Cape to the Congo, except by Mr. Daniell, whose return to England, after multitudinous and perillous adventures, was announced about the middle of last year, and who has since published a part of the information he has collected. It is particularly valuable for the means of comparison which it affords between the negro tribes of Angola and

the Western Coast, with the savages of the more northern districts. Its details upon the slave trade, which appears to be on the increase, are very interesting to the legislator and philanthropist, but do not enter within the domain of geography.

To be taken in conjunction with the work of Mr. Daniell, is an account that may be expected shortly in more copious detail, if it has not already assumed that shape, by Mr. Méquet, an officer of the French marine, of the river Gaboon (which joins the Atlantic about the latitude of the equator) and the regions adjacent.

Farther up, but yet within the limits of Mr. Daniell's observations, comes the territory of Dahomey, into which, in 1845 and 1846, Mr. Duncan penetrated more deeply than any other European traveller, and of which he has recently given an account. Nothing damped by the misfortunes of the Niger Expedition, from which he was one of five who survived, nor by his own physical sufferings during and after his escape, he offered his services to the English Geographical Society, to scale the Kong Mountains from the S. W. Coast. Under such auspices he accomplished his undertaking, and the scene of his explorations derives fresh interest from his narrative.

A communication, early last year, to the English Geographical Society, by Mr. Ingram, an officer of the British marine, upon the river Gambia, and describing his ascent of that river to nearly the Falls of Barraconda, is principally interesting in its diplomatic relation. The object of Mr. Ingram, in his official capacity, was to make arrangements with the native chiefs along that river, both for the advancement of commerce and for the suppression of the slave trade.

This brings us back to nearly the point whence this notice started, the theatre of Mr. Raffenel's departure. Northward, for many a weary mile, the arid waste of the Sahara repels the sailor from its coast; seen from the mast-head, or trodden by the foot, it looks a restless sea of sand, or the impress of that ocean which bathed long ago the bases of Atlas. Yet it is crossed, and in its widest part, though swiftly and with trembling, by anxious caravans; thus Tripoli keeps up a rude commerce with Ashantee, and the traders in slaves, of Tunis and Morocco, meet in Timbuctoo. But the circumstances of such a transit are unfavourable to the patient investigations of the scientific traveller; and, indeed, nature seems to have left nothing there except a few stray oases to be investigated at all.

Our knowledge of Morocco has received no addition, except in the publication of the journey there in the spring of 1845, of the Prince of Löwenstein-Wertheim: the exploration of the Atlas chain—one of

the most attractive fields to those who would delight in verifying ancient traditions by existing memorials—still remains unfinished.

The French establishments in Algeria have been recently noticed and explained in a compendious Memoir by Mr. Roux de Rochelle; a French officer, Mr. Pricot de Saint Marie, has continued his researches into the topographical antiquities of Tunis; and, finally, Mr. Richardson has published his Travels during 1845 and 1846 in the Great Desert of the Sahara, including descriptions of Ghadamis and Mourzouk, and the oasis of Fezzan. But what has been so far given to the world is properly the miscellaneous part of an intended work; the scientific portions, the maps, and the geographical discussions, are yet to be looked for.

The immense scientific exertions in Egypt, during the few past years, seem to have been followed by a sort of lassitude, at least a cessation. It only may be mentioned, that the classical work of Mr. Bunsen, Egypt's Place in the World-History, has lately been made accessible to English readers in their own tongue.

Such are the chief particulars belonging to the geographical notices of the continent of Africa.

2. *America.*

This continent has been now for many years, the theatre of efforts to discover a north-west passage through the ice-bound Polar regions, from the Atlantic to the Pacific Oceans. But during the present period, the existing Arctic expeditions have been rather for *re*covery than *dis*covery. The one that sailed under Sir John Franklin, and British auspices, in 1845, has not been heard from since its departure; it is imposible to say whether it should be classed among existing explorations or not. At the time of its going out, its commander did not expect to communicate with his friends at home, even in the event of full success, before October, 1847; but six months before that time, the public speculations about his fate begun to wear the aspect of alarm, and various unauthorized rumours found their way into the channel of the press, as to new expeditions under the authority of the Government to search for, and if possible rescue him. Anxious persons were still further dissatisfied with there having been no official announcement of Sir John Franklin's instructions, as to the route he was to pursue; the absence of information on this point, which without doubt was an oversight, gave wider scope to the activity of conjecture; and the stimulus of curiosity perhaps as often animated correspondents of the public prints, as did the pangs of fear or regret.

So effective, however, were such correspondents, that in June, 1847, a party of fifteen picked men were despatched from England, with suitable supplies, to winter on the Mackenzie river, and an authorized assurance was given, that if doubt still existed in 1848, that veteran explorer, Sir John Richardson, would join them with an additional party, and lead the search.

It is not within the scope of the present notice, nor does the want of an accordant map of the real or supposed Arctic discoveries, allow us, with intelligible precision, to state the various localities assigned by different authorities as the probable scene of Franklin's failure or despair, and the grave even, (for such was the emphatic language unsparingly used,) of the 125 men under his command. Two of the most competent judges, Sir John Ross and Dr. King, appear to agree in regarding the western coast of North Somerset as the most probable locality.

And the various views as to the character of the new expedition, all of which came under two general classes, viz. by water or over land, were met, or at least comprehended in the adopted plans of the Admiralty, as announced about the close of last year. 1st. A vessel was sent out immediately (December, 1847,) to Behring's Straits: parties from which, in boats and on the ice, are expected fully to explore the tract from the Straits (say Icy Cape), and the mouth of the Mackenzie. 2d. Two vessels have, in this year, been despatched, under Sir James Ross, to the eastern side of the continent; one of which, stationed in Lancaster Sound, will send out parties northward and southward towards the Greenland coast and North Somerset, while the other, passing through Barrow's Strait, and sailing as far west as Banks' Land, will, in like manner, send out parties in all feasible directions. One of these parties going southward, will, it is expected, intersect with the remaining expedition under Sir John Richardson, which, 3d, composed in part of the picked men already mentioned, whose safe arrival at York Factory on Hudson's Bay, and subsequent departure for the interior, to winter at Cumberland House or on the Saskatchewau river, was known at home; and in part of an additional force and supplies, constitutes the over-land expedition. Their purpose is to track the coast to the eastward of the Mackenzie, to examine Wollaston land, and of course to collect, among the native tribes of Esquimaux, all information of either immediate or permanent interest. In June last there was intelligence that the second detachment, and the leader of both, had reached Lake Superior, and departed to overtake and join the first. When the season closes,

their winter-quarters will be at Great Bear Lake, on the Coppermine river; and should their research be fruitless, then, and perhaps in any event, they are to resume, in the spring of 1849, the examination of Wollaston and Victoria lands.

Such are the details of the plans proposed, and so far executed for the rescue of the unfortunate navigator. No expression will be expected here as to the propriety or probable success of these comprehensive but rather long-delayed and cumbrous measures. As the carrying out of the instructions will be something very like the solution of the problem of a northwest passage, we may hope that these united exertions, though they may come too late for the relief of the gallant and unhappy Franklin, will yet contribute materially to the map of our Arctic geography. There appear to be many, however, who yet believe that such contribution will be made by the adventurous officer himself.

In connexion with the question just now alluded to, between the relative merits of marine and over-land expeditions for polar discovery, must be mentioned a very interesting and luminous paper before the Geographical Society of England, by Admiral Wrangel, on the best means of reaching the Pole.

As a member of the over-land expedition under Sir John Richardson, went Dr. Rae; whose explorations, between July, 1846, and August, 1847, of Melville Peninsula and part of North Somerset, have acquired for him a just reputation, and furnish a prominent item for a notice like the present. The main point alleged to be established by this expedition, under Dr. Rae, is the continuity with the main land of Boothia Felix, and the peninsularity of North Somerset. Even if this, in consequence of the season and methods used, should not be held conclusively settled as yet, the work itself will still remain as a remarkable monument of how much may be accomplished by the resolution and exertions of a few, and as a pattern in many respects of the plan best to be pursued in future Arctic examinations. Mr. Wyld, of Charing Cross, London, is the publisher of a sheet sketch of Dr. Rae's route and results; which will serve as an interesting morçeau for geographers, until the discoveries themselves are allowed to take their place in authentic and permanent maps.

Expeditions like these are absorbing enough, to cause other and less exciting portions of territory to be overlooked. Hence, except a memoir by Mr. Wittich, on the Peninsula of Garpi, which is the northeastern part of Lower Canada, north of New Brunswick, and lying between the St. Lawrence and the Ristiquiche, and concerning

which our knowledge has been both scanty and vague, we are without additional information as to British North America.

The systematic operations, which have been carried on for many years in the survey of the coast of the United States, and which, both in the plan adopted for their development, the combination and creation of means for their execution, and the skill and success in the use of those means, may justly be regarded as among the most important contributions ever made to scientific geography, have been regularly continued. At the date of the last report, Oct. 1847, sixteen maps of different portions of the coast had been issued; to which seven more may probably be added in the count at this moment. Every State bordering on the Atlantic or Gulf of Mexico, is now the theatre of some portion of these operations; and the entire line of coast, from Maine to Texas, with the exception of the peninsula of Florida, is involved in some one or other of the steps of the group. The use of steam for the vessels of the hydrographic parties, and for others, is vastly accelerating the execution of the work which is so dependent upon easy and prompt change of place; and the electro-magnetic telegraph has been made to wing from point to point the news even of their longitudes.

The additional extent of coast on the Pacific which the United States have obtained under recent arrangements with Mexico, is of too recent acquisition to have been placed, as yet, under similar auspices. A scientific commission, only, composed of officers of the army and navy, is about to be despatched to the coast of California and Oregon, to examine its capabilities for attack and defence. A member of such a commission could avail of its occasions for making very valuable contributions to our geographical knowledge. How far such avail is contemplated, or provided for in the selection of suitable associates, if need be, from civil pursuits—is not known.

In point of fact, it is to a similar avail of incidental opportunities, that we owe nearly all the information at present possessed about the interior of this territory, and its practicable connexions with the eastern portions of the continent. Three reports have been recently made of this sort, by officers of engineers; one by Mr. Abert, containing valuable details on the natural history of part of the region, and accompanied by a map of the course of the Rio del Norte, between the 34° and 37° parallel of north latitude; and the two others respectively by Messrs. Emory and Frémont. The former of these will shortly be despatched to execute the demarcation of the new boundary line between Mexico and the United States; in the course of which may be

expected the development of fresh geographical facts: and the latter is already en route to connect and continue, under the authority of the Senate of the United States, and with suitable scientific assistance, the explorations which in past years he so faithfully commenced and carried on. Should the government see fit to add to this, or to some other commission, suitable means of inquiry into the agricultural, mineral, and other statistical resources of the region, which is not improbable in view of the great popular interest which would be in such manner so directly promoted, geography also may be expected to realize immediate and important advantages.

Such advantages have been realized, for instance, in the official explorations carried on for geological purposes on Lake Superior; where the development of immense deposites of copper, besides enhancing the price of the soil, has also given a great stimulus to works of that metal: and so on the Pacific side, it is highly probable, especially since the discovery of coal on Vancouver's island, for the Hudson Bay Company, that the researches of surveyors would be rewarded in finding that fuel within the American territory, and accessible from tide-water.

The writings of Coulter and of Ruxton, giving account of their respective adventures among the Rocky Mountains and in Mexico, whatever they may be in other respects, are too miscellaneous to come properly within the catalogue of geographical researches, nor can we include in it the numerous books upon Texas, with which the German press has literally groaned: but the volumes of Mr. Dunlop, upon Guatemala and Central America—a region whose antiquarian richness Mr. Stephens first opened to us—are strictly within that category. A melancholy interest attends their perusal; in that the author hardly survived to see his printed pages, or to enjoy the good opinions they had won.

The geography of the Isthmus of Panama has been treated of in a short but satisfactory article by Mr. Hopkins, an English engineer, resident in New Granada; and almost at the moment of writing, the public news conveys the intelligence of the authorization, under that Republic, for the construction of a rail-way by a company, whose agent is Mr. Klein, from Porto Bello, or the Bay of Limen, to Panama.

South America has been illustrated by the journeyings, since 1842, of Mr. Castelnau, who returned to France in 1847, and whose immense collections are reported to attest at once his activity and success. The publication of his researches, in a permanent and acces-

sible shape, may be expected as soon as the affairs of France allow business to revert to its ordinary and habitual channels. There was a rumour, which, however, has not been fully confirmed, that some at least of the papers of the unfortunate D'Osery, one of the engineers accompanying Mr. Castelnau, whose assassination, by the Indians, excited so much regret, had been recovered.

Another traveller, under the auspices of the same government, Mr. Demersay, has also returned from South America, after explorations carried on there extensively for several years. More fortunate than Mr. Castelnau, Mr. Demersay has succeeded in entering Paraguay; and brings back with him, among other things, the piquant results of nearly a year's study of that region, whose state and government has been so long an inaccessible problem.

The return of Mr. De Saint Crieg, still another French traveller, cotemporaneously with Mr. Castelnau, and at one time attached, we believe, to the same expedition, is also announced; and the fruit of his researches is looked to confidently as a rich harvest for science.

Among the explorations of special districts may be mentioned those of Mr. Schomburgk, executed between 1840 and 1844, in British Guiana, and just now published. The missionary labours of Mr. Bernau bring down the scene to a year later; but in a book of its title we naturally would not look for much scientific classification or research.

Brazil has been unfortunate. Besides the recent account of the travels there in 1836—1841, of Mr. Gardner, in which considerable scope is allowed for zoology and other branches of natural science, Mr. Auguste de S. Hilaire has given to the world the third and complementary part of his work on the scientific history of that country.

With these particulars may be closed the account of this continent.

3. *Asia and Oronesia* (*the Indian Archipelago*).

The explorations in this continent are so extensive that, to give to each of them a notice, proportionate to their aim, the interest that they inspire, or the probable value of their results would swell too much the brief sketch which is at present designed. They must, therefore, be grouped together hastily, and, perhaps, with a want of apparent connexion.

Entering by the classic land of Anatolia, we find Mr. Tchihacheff penetrating the hill districts of the ancient Phrygia, which bound the valley of the Meander, and where European travellers have hardly reached before, and about to complete his three years' labour by a

geological map of the peninsula. Farther south, the coasts of Tyre and Sidon, and the adjacent mountains, have been made the subject of examination, by Mr. Newbold; and the sacred waters of the Jordan, and the mysterious bosom of the Dead Sea, have been visited and floated on by no less than two enthusiastic explorers. The earlier, Mr. Molyneux, an officer of the British marine, confirms, in a measure, the general idea, long existing, of the mephitic character of the Lake of Sodom; but he did not survive his episodical expedition long enough to edite a full account of his observations. The last, Mr. Lynch, likewise an officer, but of the American navy, has as yet only had opportunity to furnish to friends at home, the generalities of his results. Among the most striking of these is the remarkable difference of level of the two districts, so to speak, of the bottom of the Dead Sea; and the occurrence at Usdom, near the ancient Sodom, of a pillar of rock-salt, which, tiled and hooded with a layer of limestone, has been able to resist atmospheric influences, and to preserve, for thousands of years, the awful memorial of disobedient curiosity.

Another American, Mr. Thomson, who does not disdain to lighten, sometimes, his missionary labours for the spread of religious knowledge, by interest in, and observation of, the surrounding localities, has been rewarded by the discovery at Rubla (the former Riblah), near the source of the Nahr el Asi or Orontes, of a remarkable monument, covered with well-executed figures, and perhaps inscriptions, which is supposed to belong to the Assyrian period of history.

Should this be verified, it will result in giving a still farther western extension to that system of monumental remains which has been developed, as occurring in the valley of the Tigris, and even into Eastern Persia; and which, first detected by Schultz, at Van, has been confirmed by the exhumations of Botta and Hector at Khorsabad, and of Layard at Nimroud, and by the brilliant decipherment of Rawlinson at Bisutown. It is not within the scope of this notice to dilate upon the philological labours and successes of Silvestre de Sacy, of Burnouf, and of Westergaard, Grotefend and Lasren, in communication of these discoveries. We cannot, however, but pause to admire the acuteness which, out of symbols, at first sight so arbitrary and impracticable, has discerned and demonstrated the expression of at least three great languages, and, with more than Amphionic skill, has made the rocks themselves to utter the speech of Darius. All these inscriptions are in one similar cuneiform character; but those at Van, it seems probable, belong to still a fourth tongue. The architectural reliefs and incrustations recovered by Mr. Botta, are lodged safely in

Paris, whence a magnificent publication, under the auspices of the French government, is progressively illustrating their design. Those of Mr. Layard, whose disinterment, due at first to the zeal and liberality of Sir Stratford Canning, has been prosecuted, subsequently, under British national authority, as well as the gleanings which Mr. Hector made, more recently, from the harvest field at Khorsabad, have reached the British Museum. Of these last, only occasional, short notices have been given by Mr. Benorin; but a satisfactory resumé may be shortly expected under the same auspices through which they were acquired; and, very recently, the immediate publication has been announced, on the private motion, we may suppose, of Mr. Layard, of fac-simile drawings of some objects of the collection, whose material has exfoliated or disintegrated under the influence of the new atmosphere to which, after 2500 years of uniform seclusion, they have been exposed. It may be remarked, that the slabs upon which the reliefs and figures occur, and which, in their normal position, had been encrusted upon unburnt bricks, are, although in the usual notices, called *marbles*, more properly alabaster; at least they are sulphate (not carbonate) of lime, a much more perishable material than marble. When the decipherment, now in brilliant progress, shall have been completed, and the consentaneous harmony of the sculptured and painted emblems demonstrated, which there is every reason to anticipate, the next generation will enjoy a light upon ancient history and ethnology, which, at the beginning of this, it would have been hopeless to expect.

The mathematical and physical geography of a district, neighbouring on these localities (Kurdistan), has been recently illustrated by a diligent and competent observer, Mr. Hommaire de Hell; who, in the latter part of 1846, passed through, on his way to the scene of more extended and systematic explorations, which he purposes making in Independent Tartary, east of the Sea of Aral; and in which he is actually engaged. His researches will hardly reach sufficiently northward to connect with those of the Russian expedition, or of Mr. Castren; so that, in the country of the Kirghis Tartars, and in the upper valley of the Ob, there will still remain unexplored districts, to tempt future travellers. The eastern slopes of Caucasus, with Circassia, have been recently described, in part, by Mr. Wagner, upon observations made by him, from 1843 to 1846.

The Russian expedition, just mentioned, is the earliest effort of the new Geographical Society of St. Petersburg; which came into existence under the impulse given by the return of Mr. Von Medden-

dorff, from Northern and Eastern Siberia. Under the presidency of the young Grand Duke Constantine, and animated by the vigour of the imperial government, it may be expected to contribute much to science. At present, an expedition, commencing about May, 1847, under Col. Hofmann, already experienced in Siberian travels, surveys the flanks and the crest of the Ural Mountains. Starting from Serm, on the Kama, parties will simultaneously examine the Patchura, toil along the presumed passable summit of the Range, and explore the tributaries of the Ob; thus binding up, in an authentic and defined boundary, Europe and Asia. The season of 1847 was supposed to be sufficient for their reaching the 65th parallel, the northern limit of the excursions of Strajesski, who accompanies the expedition as its second head; and, in this year, they hope to reach the Arctic Sea.

The explanations of Mr. Castrèn, a young and enterprising scholar of Finland, not undertaken with so strict a view to mathematical geography, may yet be expected to contribute to our knowledge in this regard; while, on their enormous theatre, will assemble themselves most ample and valuable details and materials for ethnology. Under the auspices of the Imperial Academy of St. Petersburg, he has been engaged since June, 1845, in a series of researches, and residences at favourable spots, more or less prolonged, which have embraced the basins of the Ob and the Jenisči, and had carried him, twelve months ago, across the Altaï chain, and over even into the territory of China—the sources of the great Siberian river, and of its population.

Within the domains of this empire, as in Corea, parts of Mauchouri, and even in Eastern Mongolia, geographical information has been contributed by the labours and travels of the missionaries. Among these, the achievement by two worthy fathers, Huc and Gubet, are particularly to be distinguished, not only as distancing all others, by comparison, but as in itself an immense and appalling undertaking. Charged with the propagation of the faith westward, in the course of 1844 to 1846, these intrepid preachers had proceeded in penetrating to L'Harra in Thibet, the sacred abode of the Grand Lama himself. There, the religion and policy of the natives, shocked at their assurance, and perhaps even moved by it, sent them back safely to Canton. The detailed relation of this extraordinary journey is promised shortly; and, although unattended by appliances for strictly scientific results, it can hardly fail to be of great interest to physical geography.

The great Thibetian plain of Central Asia, unknown to Europeans, is now being assailed by other adventurous travellers, better supplied with special scientific aid, from the side opposite to that entered by Messrs. Huc and Gubet. Three gentlemen, one, Mr. Cunningham, an officer of engineers, and son of the well-known Scottish poet and artist; another, Mr. Strachey, also an officer in the service of the East India Company, and already distinguished by his successful adventure, in 1846, to reach the Mansorawara Lake, the source of the Sutledge; the third, Dr. Thomson, a son of the eminent chemist, have been employed by the East India Company, charged with instructions from the Asiatic societies of Calcutta and Bombay, and furnished with requisite instruments and apparatus for exploring and defining the topography, hydrography, climate and productions of part of the Himalaya range. Leaving Simla in August, 1847, the combined party were to ascend the Sutledge and enter Thibet through the pass by which that river has pierced the mountains. Thence, separating, Mr. Cunningham, with one party, was to proceed westward, along the course of the Sinde or Indus, and following it through the unexplored regions of Kaschmir, was to descend among the comparatively better known country of the Punjab. Another party, under Mr. Strachey, would revisit the scene of his former labours, and thence, crossing over into the valley of the Sanpoo, trace it along the southern frontier of Thibet, and finally determine the question yet unravelled, whether this last river is a continuation of the Brahmapootra, or whether it does not, perhaps, pour into the Irawaddy. The third party, under Dr. Thomson, is devoted to the investigation of the natural history of the mountain range itself.

It is possible, that with this last, may intersect another expedition, already on foot, under a distinguished British naturalist, Dr. Hooker, and supported by the British government. Sailing from England, in Nov. 1847, in a steam-frigate, for Calcutta, Dr. Hooker is to spend a few months after his arrival, in investigating the botany of Bengal, and then to betake himself, with the same view, to the southern slopes of the Himalaya. If time allows, he will extend his excursion across into Thibet. He is expected, in 1849, to return to Calcutta; and thence visit, upon a similar exploration, the newly ceded British island of Labuan, on the west side of Borneo; and, finally, Borneo itself, in which the genius of Mr. Brooke has created a new State. It will be observed, from this, that the scientific object of Dr. Hooker's expedition is especially botanical; but the opportunities he will have

for observation will not fail to be utilized for other details of geography.

Before quitting this region, must be mentioned the quite recent publication of Baron Hügel—Kaschmir and the Empire of the Siekh; and the name of Sutledge recalls the melancholy fate of another traveller, who perished there, and whose reputation, like that of Mr. Dunlop, already mentioned in the notice of thé American continent, is, so far as rests on publication, posthumous. This is Dr. Hoffmeister, an attendant of Prince Waldemar, of Prussia, whose Letters from India, not long published, cover an extensive tour (even only so far as India is concerned), from Calcutta through Bengal, into Nepaul, over the Himalaya, and thence by the pass of the Sutledge to Simla, and only make us regret the more the issue of the gallant day at Firozeschah.

The publication of the operations, from 1830 to 1843, for measuring sections of a meridional arc in India, extending for the extremes, from about 18° to 29° 30′ of latitude, by Captain Everest, under the authority of the East India Company, adds another to the list of systematic geoderic works which the present century has produced. Although it may not compare, in point of absolute accuracy of result, with some others—for instance, the American survey, mentioned a little while ago—yet it is to be remembered, that it encountered and overcame local and attendant difficulties from which these others were exempt; and the indefatigable ingenuity of Capt. Everest, in adapting unfit instruments to his purpose, and his remarkable dexterity in their use afterwards, raise him to a very distinguished rank among geoderists.

The territory of Assam, since the Birman war, has been so little heard of, that it seems as if it had been wiped out by the elbow of the Brahmapootra. In the return of Mr. Robert, a French traveller, who, two years ago, was preparing to follow the course of the Sinde, and examine the upper districts of the Punjab, but who since returns to Trauce, after revisiting Assam, we may expect some valuable contributions to our knowledge. An interesting publication has, indeed, been recently made, of miscellaneous details relating to the geography, manners and customs and business statistics of the region, with copious and curious sketches and traditions of the hill tribes of Assam, by a military officer, on civil service, at Burpetah, who, while he accepted a service that the terror of jungle-fever allows but few to take, has withheld his name from a publication in nowise tending to his discredit.

The principal addition to the geography of China proper, has been in the account by Mr. Collinson, of the royal navy, of the exploration of the Yang-tse-kiang, by the squadron under Sir William Parker, in 1840. Several miscellaneous narratives have also been published, such as that of Mr. Fortune, in which, among a good deal of solely personal adventure, occurs also interesting notices of the botany of South-eastern China; and of Mr. Meadows, whose apparently familiar knowledge of its peculiar language (or rather languages), entitles to a good deal of weight, what he says about, not only philological questions, but also the government and people of that country, however his statements may differ from those habitually made and accepted. The account of the five years' residence there, from 1842 to 1847, by Mr. Forbes, an officer of the British navy, is without much accessory advantage of the sort; and a part of it links itself with other localities, in the narrative of the occupation of Labuan and Borneo.

This last locality has, indeed, quite a literature of its own; grown up out of the materials furnished, and the impulses given by the Rajah of Sarawah, whose destiny is as oriental as the climes to which he has chosen to devote himself. In less than ten years from the time he published the prospectus of his design upon the Indian Archipelago, Mr. Brooke has worked wonders, not so magically instantaneous as those of the lamp of Aladdin, but more diffusive and enduring in influence and effect. His genius, if it finds its sphere amid only the heat and glare of an equatorial sun, is at least more under command, and more benevolent than the irresponsible soulless slave of the Arab's dim and flickering lamps. Since the publication, in 1845, of Mr. Keppel's expedition, we have a third edition, comprising recent intelligence; Mr. Mundy, a gallant and intelligent naval commander, has brought the narrative of events down to the cession and occupation of Labuan, and has given copious additional extracts from the earlier journals of the future Rajah. The talented young Marryat, whose unhappy fate struck down two lives, has transferred, in brilliant illustrations, some of the glow and richness of the Indian scenery; Mr. Low has given valuable information upon Sarawah, its inhabitants and productions, and has particularly enriched the science of botany with new contributions; and, finally, Sir Edward Belcher, in the narrative of the surveys of the Samarang, during 1843 to 1846, while giving authentic data for the reformation of our maps of the Indian Archipelago, has also furnished important material for the ethnography and

natural history of the islands. Such, and so great are the results that can be effected by the energy and patience of one man!

After an absence of very nearly nine years, Mr. Brooke returned to England, and reached Southampton on the 1st October, 1847. Honourably distinguished on every side, he made no long stay, but on the 1st of February following returned, in the Meander, to Sarawah and Labuan. The increased resources for civilized and Christian instruction, which have been promptly contributed by the mother country, cannot fail of producing the happiest effects; and the increased force at his disposal will enable him all the more promptly to suppress the Illanun pirates, and render as safe as it is beautiful, the navigation of the Indian Archipelago. Then only may we expect to see definitively removed, the confusion and vacuities which embarrass and disfigure our maps of that region.

Tributary to this end, may be mentioned the recent reconnoissance of the Loo-Choo groupe off the east coast of China, by French surveying vessels, under the direction of Admiral Cecille. Other islands remain unexplored, with exception of Java, where a short visit, in a British surveying vessel, allowed Mr. Jukes, the naturalist and narrator of the expedition, to land, and thus enables us to connect the geography of Asia with that of the great assemblage which follows—the continental islands of Australasia.

4. *Australasia.*

In this region, and especially in Australia, although it is hardly sixty years since the first white settlements were made in New South Wales, extensive explorations have been already carried on, and proportionate discoveries have accrued to reward them. The last year, in particular, has been fruitful in interesting publications of what had been executed not long previously.

For instance, Dr. Leichardt, an enthusiastic and energetic young Prussian, geographer and naturalist, has given account of his overland expedition from Sydney, on the south-eastern side of Australia, to Port Essington on the north, a distance, in a direct line, of not less than 2500 miles, passed over during 1844 and 1845. More recently, Sir Thomas Mitchell, the surveyor general of the colony of New South Wales, and the successful explorer of the Darling, has given the journal of an expedition in the same direction, and of as great extent, whose aim was to discover a feasible route from the settlements on the S. E. coast, to the Gulf of Carpentaria. This was successful in discovering a large river, to which the name of Victoria was given,

and which, doubtlessly, pours into the Gulf of Carpentaria. According to the latest intelligence, additional parties had been sent out to explore the river to its estuary; and strong hopes are entertained of the ability to establish a direct line of travel and traffic between Sydney, through the Gulf of Carpentaria and the Indian Ocean, and the frequented port of Singapore. Account has also been given of the expedition of Captain Sturt into the interior, which, although not so fortunate in its results, was yet valuable to geography. The official examination of Spencer's Gulf has resulted in details not less valuable to commerce.

It is hardly known whether a new and vast design of Doctor Leichardt is to be spoken of as among existing explorations. This was to pass over and determine the country, in a line as direct as possible, between Moreton bay on the east side, and Swan river on the west of Australia; an undertaking which, among all that have been spoken of in any part of the world, is without parallel, except in the instance of Mr. Raffenel, for Africa. The intelligence at the close of 1847 was, that the cattle which the traveller was employing, as well for the transport of necessary baggage and provisions as for food themselves, had, when reaching the western limit of civilized settlements and communication, become wild, and escaped from their drivers; thus postponing, at least, and seriously perilling the whole undertaking.*

The researches of Burnett on the Boyne, of Campbell in the interior, and of Gregory on the western side of Australia, do not yet furnish definitive data for geography.

In southern New Zealand, Mr. Brunner is reported to have started in December, 1846, accompanied by two natives, to attempt the passage from the south of Hourib lake to the open country below. In case of impracticability there, he purposed tracing the Buller river to its mouth. "On reaching the coast, he was to proceed to Araura, in lat. 43°, the termination of his journey along the west coast in the winter of 1845, and thence to cross the island to Port Cooper. From that point he would attempt to reach the heads of some one of the great southern rivers, and to strike the coast again about the southern boundary of the New Edinburgh settlement." However these de-

* Since this was written, the intelligence of the total wreck of the expedition has been fully confirmed. Nevertheless, the indefatigable explorer only awaits fresh means—the result of his own labours and exertions—to renew his undertaking.

signs may work together, their partial execution cannot but create large additional materials for the geography of a territory, whose interior is yet for us almost a blank.

5. *Europe.*

As might be supposed, the researches in this old and thickly-settled continent belong more to geology than to descriptive geography. They would require, therefore, for their results, a more technical account than is designed or suitable here. Besides, confined to the detailed examination of local and comparatively, if not absolutely, small districts, none of the results of 1847 have materially extended or altered the general principles of physical geography, as already accepted before. It is to be hoped, however, that the present indication may serve to give rise, at future meetings of the American Association, to a special assemblage of annual geological results.

6. *Oceanica.*

Under this head, which I propose to consecrate to the observations and inductions concerning the tides, winds, storms, &c.—such important elements in the discussion of physical geography—there remains to be mentioned, principally, the results of Captain Beechey, R. N., from experiments made on the tides in the Irish sea. This observer appears to have established not only the existence, but the *loci* of certain tidal nodes, where the influence of attraction is neutralized by the effect of accidental currents. As the explorations are extended, these last will no doubt lose their epithet of accidental, and come to be grouped in their proper systematic relation with a general theory of the actual tidal phenomena of the ocean. To this, the industrious and well-reflected efforts of Capt. Maury, of the National Observatory, in furnishing charts and digested instructions, as well for our commercial as naval marine, cannot fail materially to contribute.

The reading of this paper was followed by a communication by Dr. Martin H. Boyé—

On the Composition of the Schuylkill Water.

1. I propose to offer to the Association a few remarks on the composition of the water of the Schuylkill river, as furnished for domestic purposes to the inhabitants of Philadelphia. As known, this

water is, for the above purposes, forced by the Fairmount Water-works, in which the wheels are driven by the fall of the water itself, up into reservoirs constructed on the top of the adjacent primary hill, called Fairmount, and from these reservoirs, supplied, through iron mains and leaden service pipes, to the inhabitants. Though the water has thus, for a long time, been used not only for drinking, but also for all the other purposes for which water is required in a large city, we have of it, yet, but two analyses, both called forth by the agitated project of supplying our two sister cities, New York and Boston, with water, and mainly with a view of settling the question of the safety of leaden pipes for its conveyance. One of these was performed by myself six years ago, and may be found in the Report of the Board of Aldermen in New York, directed, I believe, to the City Council. The other was performed three years ago by Prof. B. Silliman, jr., and will be found in his report to Prof. W. R. Johnson, one of the Commissioners appointed to consider the subject of supplying the city of Boston with pure water; this latter was performed, as stated, without a knowledge of mine, and is accompanied with many of the details of the analysis; it thus affords an excellent opportunity for comparing the results, and to add a contribution towards the question of uniformity in composition of such waters at different periods, having between them an interval of about three years. I shall, also, by this comparison, have an opportunity of giving some of the details of my analysis, which, being performed merely with a view of forming a professional opinion in regard to the safety of the conduction of Croton water in leaden pipes, the details were not given at the time. By now doing so, the responsibility of its correctness will of course rest entirely with me, as the sole performer.

But before drawing any conclusion with regard to existing differences, it becomes necessary to subject both analyses to an impartial investigation, to examine the different methods by which the results have been obtained, and ascertain on what side of truth the probable errors, that might arise from their imperfection, will lie, and if possible, to estimate their amounts, where other details afford a check. No analysis should, therefore, ever be received in science, unless accompanied by such details; because not only does their omission deprive us of the means of judging of their accuracy, but it also takes from subsequent experimenters the means of avoiding the same errors, and of improving by the experience of their predecessors. In the present case, I cannot sufficiently laud the candour with which the

details of the analysis are given, which at once vouches for the care bestowed on it, and without which it would not have afforded any means of comparison. I hope that I shall be found to give the details of my own with equal candour, and that, in the following, I shall be considered to have had no other object in view than an impartial investigation of truth. I consider it also due to Prof. SILLIMAN, to state that his analysis of the Schuylkill water was performed, not by itself, but at the same time with eight other specimens, and the whole performed in an uncommonly short time, considering the difficulties of such analyses.

2. The following exhibits a tabular view of the results of the two analyses:—

TABLE I.

Analysis of Schuylkill Water.

M. H. Boyé.	Grains in 1 gallon.	In 100 residue.	*B. Silliman, jr.*	Grains in 1 gallon.
Alkaline Chlorides,	0.153	3.75	Chloride of Sodium,	0.1470
Alkaline Sulphates,	0.560	13.74	Chloride of Magnesium,	0.0094
Alkaline Carbonates,	0.185	4.53	Sulphate of Magnesia,	0.0570
Carbonate of Lime,	2.190	53.67	Carbonate of Lime,	1.8720
Carbonate of Magnesia,	0.484	11.87	Carbonate of Magnesia,	0.3510
Alumina and Oxide of Iron (Phosphates?)	0.077	1.88	Silica,	0.0800
Silica,	0.395	9.68	Carbonate of Soda from decomposition of crenates and nitrates,	1.6436
				4.2600
Organic matter,	0.036	0.88	Organic and other matter volatile at red heat,	1.2400
Total residue,	4.080	100.00		5.5000
By a separate experiment, total residue,	4.421		Carbonic Acid given off by boiling from 1 gallon, 3.879 cubic inches.	

Remarks.

Water taken from middle reservoir on top of Fairmount, Oct. 4, 1842.	Water taken from the pool of Fairmount dam, Aug. 23, 1845.
Amount operated on, residue from one gallon 3¼ pints.	Amount operated on, residue from one gallon.
Slight alkaline reaction on litmus paper.	Neutral to tincture of litmus.

By nitrate of silver, at first a scarcely perceptible turbidness; after 24 hours a very minute purple precipitate.	By nitrate of silver scarcely any visible cloudiness—by standing, pink colour; after 24 hours, turbid, by a violet pink precipitate suspended in it.
By chloride of barium, no precipitate. From solid residue estimated 0.302 grains sulphuric acid per gallon.	By chloride of barium, no precipitate in cold. By boiling, yielded precipitate, estimated 0.0245 grains sulphuric acid per gallon. By heating of solid residue, a well-marked deflagration from presence of nitrates.
By acetate of copper, at first no precipitate, after some time a minute light greenish precipitate.	By acetate of copper, no immediate precipitate; after 24 hours a light green precipitate of crenate of copper. By tincture of galls, but little effect at first; after 24 hours a well marked effect—brown colour.
Lime water, minute precipitate after some time.	Oxalate of ammonia; precipitate.

Table II.

3. The numerical results of Table I. reduced to 10,000 parts of water, and given, as obtained by analysis, without endeavouring to arrange the ingredients in the manner in which the analyst may consider them as combined in the water, will stand thus:—

M. H. Boyé.	In 10,000 parts.	*B. Silliman, jr.*	In 10,000 parts.
Sulphuric acid,	0.051775	Sulphuric acid,	0.006447
Chlorohydric acid,	0.016275	Chlorohydric acid,	0.016855
Carbonic acid,	0.220195	Carbonic acid,	0.288098
Potassa and soda,	0.076723	Soda,	0.178483
Lime,	0 211350	Lime,	0.180658
Magnesia,	0.040095	Magnesia,	0.033088
Alumina and oxide of iron (phosphates?)	0.013200		
Silica,	0.067710	Silica,	0.013710
Organic matter,	0.006170	Organic matter,	0.212600
	0.703493		0.929939
Deduct water in chlorohydrates,	0.004022	Deduct water in chlorohydrates,	0.004174
Total residue in 10,000,	0.699471	Total residue in 10,000,	0.925765

4. Of the amount of solid residue and its relative proportions of volatile and fixed matter, and also of the relative amounts of this latter,

soluble and insoluble in water, Prof. SILLIMAN made a separate estimation. For these same, I find also an estimation by Prof. HORSFORD, of Cambridge, (see the Report of the Boston Water Commissioners to City Council, page 33), but it will soon be seen that it is of no use, as it evidently contains some error. According to him, 500 cubic centimetres yielded, by evaporation in water bath, 0.3007 grammes of solid residue, of which 0.1032 were expelled by ignition, and 0.1975 left, of which again 0.0239 grammes were insoluble in water. This estimation would yield 35.18 grains of solid residue to the gallon.

The following exhibits a tabular view of the different amounts of solid, fixed, and insoluble residue obtained by Prof. SILLIMAN and myself:—

TABLE III.

	Boyè.		*Silliman.*	
	Grains in 1 gallon.		Grains in 1 gallon.	
Solid residue,	4.080 4.421	at 250° or over.	5.50	at 212°.
Fixed at a red heat,	3.794	calculated	4.26	
from Table I.			3.69	by direct experiment.
Insoluble in water,	2.896	„	2.145	calculated from Table I.

5. It may be remarked, that the accurate estimation of solid ingredients in natural waters is very difficult, and in many cases impossible by direct experiment. This is the case when the waters contain chloride of calcium and magnesium, and much organic matter. By heating the residue of such waters in a water bath, before weighing it, it is extremely difficult, if not impossible, to expel all hygroscopic water from the organic compounds; while, if we raise the heat around the containing vessel much above the boiling point, we run the risk of expelling from them more than their hygroscopic water. When the residue contains, beside organic compounds, chlorides of calcium and magnesium, the water expelled at these temperatures is never constant, but varies with the time during which the residue is kept heated. The best method to estimate it by direct experiment, where the balance allows of this method, I consider to be, to evaporate by the aid of a water bath in a small counterpoised porcelain or platinum capsule or crucible, that will hold from 500 to 1000 grains of water, this quantity several times; then to expose the whole residue to a temperature of about 250°, and having allowed it to cool under cover in a receiver over sulphuric acid, to weigh it quickly. In the above analysis of mine, the water, one gallon and one and a half pint, was

evaporated in a moderate sized capsule over a gas lamp, replenishing it as the water evaporated, till a sufficient quantity had been introduced. It was then evaporated to perfect dryness, finishing the operation in a water bath, and then transferred into a crucible by a platinum spatula, having moistened it barely sufficiently to allow it to be removed without flying. What then yet adhered to the capsule, was loosened by adding more distilled water, and rubbing it with the spatula or a feather till it became perfectly loose, then washing it down into a corner of the capsule, again evaporating the water, and transferring the residue; repeating this same operation till the whole residue was effectually removed. It was then heated considerably above the temperature of boiling water. This method is, however, objectionable, for with the utmost care it is impossible to get all the residue removed, and more especially the silica. This same objection applies still more to the method of Prof. SILLIMAN, of evaporating the water in a number of glass flasks, and transferring their contents, as they became concentrated, into one, and from this into a small counterpoised capsule, for some precipitate is always deposited during the concentration. The second estimation of 4.421 grains to the gallon was obtained by a separate experiment, performed by a single evaporation in a small capsule, but having only a few hundred grains left of the same lot of water from the analysis; this circumstance renders also this estimation less certain than it otherwise would have been, though the balance employed was of a superior delicacy.

6. The difference of the two analyses in the total amount of solid residue of one (wine) gallon of the water, is 1.08 grains and 1.42 grains. As I heated the residue to a higher temperature, the actual difference is less than this amount, and seems to be due mostly to the amount of organic matter, which I have estimated at only 0.036 grains, while Prof. SILLIMAN's estimation, deducting the carbonic acid, equivalent to all the magnesia, 0.207 (see § 7), gives it 1.03 grains, or a difference in organic matter of about 0.99 grains. I am under the impression that my estimation is too low, as I, on the whole, from want of time, did not pay as much attention to the organic matter as it deserved, and I have often observed that the organic matter in the Schuylkill water, at certain seasons, undergoes a change or kind of fermentation by standing in an open vessel, indicated by a deposit of a green film on the bottom of the vessel. As the water analysed by me was filtered, the above difference would be somewhat reduced, if Prof. SILLIMAN did not first filter his, as small animalcules and other very fine organic matters often remain suspended in the

water without settling. Whether the remaining difference must be explained by an actual difference of the amounts of organic matter in the water, at the two different periods, I leave undecided.

7. The difference of the two analyses in the amount of inorganic ingredients, taken as a whole, varies less than the whole residue, including the organic matter. By direct estimation, Prof. SILLIMAN found the fixed ingredients (see Table II.) to be 4.26. By calculation from my analysis, for I did not ignite the residue before analyzing it, (deducting from the inorganic ingredients the carbonic acid 0.250 grains, of the carbonate of magnesia 0.484 grains), the fixed inorganic ingredients amount to 3.794, making a difference of 0.466 grains to the gallon. Calculated from the same data, in Prof. SILLIMAN's analysis, the difference is still greater, but as this is evidently intended to represent and make up the amount of 4.26 grains obtained by direct weighing of the residue left after ignition, (see his Report, page xvii.), an oversight has been committed in representing it to contain carbonate of magnesia, which, by a red heat, loses its carbonic acid. An error also exists in the summing up of the ingredients, (see Table I. or his Report, page xx.), for instead of 4.26, they will be found, by addition, to make only 4.16. This difference (0.1 grain), together with the equivalent carbonic acid (0.207) belonging to the carbonate of magnesia (CO_2=0.1815), the chloride of magnesium (CO_2=0.0044), and sulphate of magnesia (CO_2=0.0207), (for these two latter can certainly not exist at a red heat, together with carbonate of soda), leaves 0.307 grain to be yet accounted for in the analysis.

8. This difference of 0.307 grain, distributed on the different ingredients in proportion to their relative amounts, would raise the amount of carbonate of lime, in Prof. SILLIMAN's analysis, to 2.010 grains, and that of the carbonate of magnesia, (substituting also, instead of the chloride of magnesium and the sulphate of magnesia, their equivalent of carbonate), to 0.521 grain, thus reducing the difference in the two analyses, for the carbonate of lime, to 0.18 grain, and for the carbonate of magnesia, to 0.04 grain, rendering the agreement in the total amount of these two main and most important ingredients, constituting nearly two-thirds of the whole solid residue, complete to within 0.14 grain, to the gallon (see also § 10).

9. The amount of alumina and oxide of iron has not been determined by Prof. SILLIMAN. Their amount is, at all events, but inconsiderable, but some importance attaches itself to it, as, if any phosphoric acid be contained in the water, it must be found in this precipitate. The reason why I omitted to examine it for this sub-

stance was, that I thought its amount too small to give satisfactory results.

10. In the silica, the apparent difference in the two analyses is considerable. From the method employed, and subsequent experience from other waters, in some of which I have found as much as 50 per cent. of silica, I am convinced that my estimation, if anything, is too low. With all possible care, some of the silica will attach itself so firmly to the vessel during the evaporation, that it becomes perfectly impossible to remove it completely by rubbing with water, or even with acids. This same cause would act still more to diminish its amount in Prof. SILLIMAN's analysis, where the portion precipitated, during the evaporation in the glass flasks, could not be removed by any mechanical means. That the actual amount of silica in the water analyzed by Prof. S., must have been greater, can easily be proved by his own experiments; for if we, from Table I., add together the carbonate of lime, the magnesia and the silica, they ought, together, to make up all the fixed insoluble ingredients; but in this way we only obtain 2.145 grains, while his direct estimation of the same, (see Table III. and his Report, page xvii.), yielded 3.69 grains, a difference of 1.54 grains. Allowing a fair proportion of this to be due to the too small estimation of the carbonate of lime and the carbonate of magnesia (see § 8), it leaves yet a difference for the silex in the two experiments, which is too great to be supposed to have been committed in that one ingredient, and shows that part of the difference must be due to the separate estimation of the whole insoluble ingredients. The amount of insoluble fixed ingredients calculated in the same manner from my analysis, gives it 2.9 grains. The average of the two above numbers of Prof. S., will give it 2.92 grains.

11. It has already been seen, that some great mistake must exist in Prof. HORSFORD's estimations of the amounts of solid ingredients in the different waters, making the Schuylkill water contain 35 grains of solid residue to the gallon. From the foregoing, it appears that his estimation of the relative proportions of fixed and insoluble matter, is equally erroneous. The main ingredients of the fixed residue of the Schuylkill water are formed of carbonates of lime and magnesia, with some silica, constituting the insoluble and greater part of it, (according to the above analysis, at least more than 50 per cent.). How Prof. HORSFORD can make it to be only 12 per cent., (0.0239 out of 0.1975, see the Boston Water Commissioners' Report to the City Council, page 33), is utterly inconceivable, more especially as it is evident, that he did not heat the residues to the expulsion of the car-

bonic acid from the lime, or that he means, by "insoluble after ignition," insoluble in acids; for he states expressly, that these amounts, "insoluble after ignition," "are, for the most part, carbonate of lime."

12. The amount of chlorohydric acid, and its equivalent of chloride of sodium, is almost identical in both analyses, the latter varying only 0.006 grain to the gallon.

13. The difference in the amount of sulphuric acid, though small by itself, is nevertheless considerable, when compared with its quantity. As estimated by me, from a portion of the residue (the same that was used for the estimation of the chlorohydric acid), it amounts to 0.302 grain per gallon. Prof. SILLIMAN estimated its amount by precipitation of the water during boiling, without previous concentration, to be 0.0245 grain per gallon, making a difference of 0.277 grain per gallon, and being less than one-tenth of the amount found by me. Calculated from the analysis in Table I., the amount is a little different (0.0376). It is therefore uncertain whether Prof. S. also estimated it from a part of the solid residue, as he makes no mention of it. If not, I should consider his method likely to give a too small result, for as the gallon, or 58,330 grain, can contain only .03 of a grain, the water contains only $\frac{1}{194000}$ of its weight of sulphuric acid, which is rather too great a dilution for forming and collecting, completely, a precipitate, of however insoluble a nature it may be.

14. The amount of alkali in my analysis, is 0.448 grain to the gallon, in Prof. S.'s 1.041 grains to the gallon, making a difference of 0.593 grain to the gallon. Prof. SILLIMAN gives the whole as soda, not mentioning whether he tested it for potassa. Though I did not estimate its quantity, I convinced myself of its existence. Prof. S. also considers all the carbonate of soda to have been formed from the decomposition of crenates and nitrates, but I am confident that the water contains free alkaline carbonates, and that if it be evaporated to dryness in a water bath, and again treated with a small portion of water and filtered, the concentrated solution, or the residue obtained from it, will show an effervescence with an acid; an experiment which, though uncertain whether performed with the residue from the Schuylkill water, I have performed with similar waters from the neighbourhood of Philadelphia. The slight alkaline reaction which I found on litmus paper, seemed also to indicate the existence of free alkaline carbonates.

15. In regard to the existence of nitrates in the Schuylkill water, it is an important observation of Prof. S., that by heating the residue, a

distinct deflagration took place; though this would undoubtedly tend strongly to infer their existence, still it would be well to confirm this by collecting or observing the nitrous oxide gas given off at the same time. To me no such deflagration occurred; perhaps I did not heat the residue to a sufficiently high temperature, though I heated it considerably above the boiling point of water, and I am under the impression that it began to turn slightly brown on the edges.

16. It will thus be seen that, as a whole, the two analyses confirm each other considerably, and I believe that the following results may be derived from them in regard to the composition of the Schuylkill water, and the differences in it at the two different periods, having three years between them.

The Schuylkill water, when settled clear, is a water of superior purity.

Its amount of chlorine and sulphuric acid is too small to produce any sensible reaction under most circumstances.

Its main characters it derives from its carbonates, more especially the earthy carbonates, which, if they were present in larger quantities, would give it the character of a hard water, and—

In regard to a difference of composition, at the two different periods, that,

1. The amount of sulphuric acid varied less than 0.277 grain to the gallon (§ 13).
2. Chloride of sodium, less than 0.006 grain (§ 12).
3. The earthy carbonates less than 0.4, perhaps not more than 0.14 (§ 8).
4. The silica less than 0.32 grain (§ 10).
5. Organic matter less than 1 grain to the gallon (§ 6).
6. Total amount of solid residue less than 1.4 grains to the gallon.

Dr. Gibbon, Dr. Hare and Prof. Johnson, also entered into some explanations relative to the character and variability of the Schuylkill water.

After the conclusion of these remarks, Prof. Henry made a further brief explanation of the objects of the Smithsonian Institution. After which, the Association adjourned, to meet this afternoon at 4 o'clock.

Monday, September 25, 4 P. M.

The Association met agreeably to adjournment. President, Wm. C. Redfield, Esq., in the chair.

The Standing Committee made a report, recommending the appoint-

ment of a committee to superintend the publication of the proceedings of the present meeting, and nominated for that committee—

Dr. ROBERT W. GIBBES, of Columbia, S. C.; Dr. ALFRED L. ELWYN, of Philadelphia; S. W. ROBERTS, Esq., of Philadelphia; Prof. JAMES B. ROGERS, of Philadelphia; WM. S. VAUX, Esq., of Philadelphia; Prof. B. SILLIMAN, Jr., of New Haven.

Which recommendation and nominations were, on motion, concurred in. The Standing Committee also recommended that 1000 copies of the proceedings of the Association be published in pamphlet form, and placed at the disposal of the Chairman of the Committee on Publication.—Adopted.

The following resolution, in regard to the publication of the proceedings, was also adopted:—

Resolved, That copies or abstracts of all communications made, either to the General Association, or to either of the Sections, must be furnished by the authors, otherwise only the titles shall appear in the published proceedings.

The Standing Committee offered the following resolution, which was, on motion, adopted.

Resolved, That this Association highly appreciate the object of Prof. GERMAIN, in collecting the fossils of the Greensand of New Jersey, and that the Society will, by every means in its power, encourage him in the same.

The following gentlemen, having been duly nominated for membership, by the Standing Committee, were unanimously elected members of the Association, viz:

ALGERNON S. ROBERTS, Esq., of Philadelphia; Dr. B. B. BROWN, St. Louis, Mo.; Dr. GERARD TROOST, Nashville, Tenn.; LEWIS TROOST, Esq., Nashville, Tenn.; Prof. A. GUYOT, of Cambridge, Mass., and JOHN E. THOMPSON, Esq., of Philadelphia.

Prof. HENRY, on behalf of the Standing Committee, offered the following resolution, which was adopted:—

Resolved, That the thanks of the Association be tendered to Prof. JAMES B. ROGERS, for his kind attention to the convenience of the Association while holding its Sectional and General Meetings in the Chemical Lecture room of the University.

On motion of S. W. ROBERTS, Esq., it was—

Resolved, That the thanks of this Association are due, and are hereby respectfully tendered to the Trustees of the University of Pennsylvania, for the liberality with which they have given to the

Association, the use of the halls of both the Collegiate and Medical Departments of the University.

Prof. W. R. JOHNSON offered the following resolution, which was adopted:—

Resolved, That the thanks of the Association be presented to the Chairman of the last Annual Meeting, for the able and eloquent address delivered before the Association at its present session; and that he be requested to furnish a copy thereof, for publication in the proceedings.

The Secretary read the following letter from Prof. TUCKER, which was ordered to be entered on the minutes:—

Philadelphia, September 25, 1848.

Sir,—Unable to leave my room, from an attack of bronchitis, I beg leave to propose what I had wished to submit this morning in person.

I have been long desirous of seeing a general Statistical Society in the United States, and last year invited, through Hunt's Magazine, a meeting in this city for that purpose. The plan was very favourably responded to by some; but their number was not sufficient to justify the immediate formation of such a society. It was then suggested, that all the purposes of such a society might be attained by a Section of this Association, appropriated to statistics and political economy. I accordingly hastened on to the meeting, from Virginia, where I was on a visit, for the purpose of proposing such a Section, and, in my journey, caught the cold which now disables me.

I will not waste the time of the Association in dwelling on the advantages of statistics. They are known to be auxiliary to all the sciences that are not demonstrative, and to furnish materials for those which are. They are particularly valuable in the United States, where changes are more rapid than elsewhere; and where, from the extent of our country, enumerations are more difficult. It may further recommend such a Section, that it will tend to make more of the proceedings of the Association intelligible and interesting to the public generally, and thus further the liberal and patriotic views of the Association; and though I do not belong to the class of utilitarians, but regard every species of knowledge valuable for its own sake, it cannot be a matter of indifference, that the pursuits of science, as those of statistics and political economy, have a direct bearing on the common concerns of life—lastly, in favour of such a Section, the Institution in England, which has served as our prototype, has a Section

of statistics. I accordingly beg leave to submit the following resolution:—

Resolved, That a further Section of Statistics and Political Economy be added to those already established in this Institution; but that the appointment of its members be postponed to the next meeting, in August.

I cannot conclude this hasty communication without congratulating the Association on the success which has attended its first meeting. The papers that have been read to it appear to me to do great honour to their authors and the Association, and afford a sure presage that the fondest hopes of its founders will be more than realized.

I am, sir, with great respect,

Your obedient servant,

GEORGE TUCKER.

Prof. Wm. B. Rogers offered the following resolution, which was adopted:—

Resolved, That the letter of Professor Tucker, on the subject of forming a Statistical Section, at the future meetings of the Association, be referred to the Standing Committee for consideration, and report at the next meeting of the Association.

The Secretary also read the following report, by Mr. Kellogg, made in obedience to a former request of the Association of Geologists and Naturalists.

A Remarkable Geological Development, in Elizabethtown, Essex County, N. Y.

During the period of the Geological Survey of the State of New York, about six years since, when the writer of this article resided in Essex county, he was requested by Prof. Emmons, who was then prosecuting the survey in that county, and had not himself then time to visit the spot, to send him in time for the annual report of that year, a description of the phenomenon alluded to above, which is regarded as a pot hole of geological origin, and which is found in the valley of the Boquette, about four miles south of the Court House, in Elizabethtown, above named. Owing to other engagements, on the part of the subscriber, whose residence was about twelve miles from the spot to be examined, it was neglected till too late for the object then contemplated; and the thought has been suggested, that such description

might not be uninteresting to members of the Association for the Promotion of Science, or inappropriate for the present occasion.

The Valley of the Boquette, known in Essex county, as Pleasant Valley, lies on the west side of Lake Champlain, runs parallel to the lake, and at the distance from it of about eight miles; and its southern extremity is a little north of west from Crownpoint. In other words, the whole valley lies directly west of the town of Westport, and is from seven to eight miles in length. At the southern termination it is a mere gorge in the mountains, into which the river Boquette falls from a rocky cliff on the west. From this point it gradually opens, for the distance, perhaps, of three miles, till it becomes, where the development alluded to occurs, and which is about four and a half miles from its northern extremity, about half a mile in width; which width it preserves nearly the same for that distance, except that it widens out into a sort of basin at its base. At this north end, where the village above named is situated, the valley is about six hundred feet above the level of the lake, and, at the place just named, four and a half miles above, it is probably two hundred feet higher. On the east of the valley is a mountain range, some seven or eight hundred feet high, and, on the west, one of perhaps twice that height.

Through this valley, from south to north, runs the river Boquette; and the whole valley has been regarded as having been probably once filled with water, forming a pond, connected with Lake Champlain by a gorge in the mountains on the north-east.

At the distance before stated, from four to four and a half miles from the lower extremity of the valley, is an elevated plain, extending quite across the valley from east to west, and embracing an area of some fifty or sixty acres. It falls off, on both sides, by an almost perpendicular bank; that on the south being, perhaps, seventy-five or eighty feet in height, and that on the north somewhat higher; the river, which passes around, under the mountains, on the east, and on which Bishop's mills are situated, making quite a fall at this place. Immediately above, and south of this plain, the valley becomes narrowed to about half its breadth on the north. The banks, on both the south and north, show very evidently that they were formed by the river: that this river, at some former period, came in from the south, along the western boundary of the narrow interval, above the plain, and veered around under this southern bank, easterly and south-easterly, quite across the valley, by a sort of horse-shoe bend, to the place where the river now runs; and then, a short distance below the fall, took again a western turn, and, by a similar bend, passed quite

across the valley, under the northern bank, to its western boundary, and thence, along the foot of the western mountain range, for some distance below.

The south-eastern extremity of this plain, beneath and around which the Boquette passes, is terminated by a hard and smooth granite ledge, while the bank, from that point, on the south, across the valley, to the west, and across the whole extent, on the north, exhibits evidence that the whole plain is but one uniform deposite of drift, consisting of sand and gravel, with occasional small boulders. Just on the top of this ledge, where it begins to slope towards the south, the cavity alluded to occurs. It forms, on the top, nearly a perfect circle, and is not far from four feet across and five in depth. To the distance of about half that depth, it preserves its kettle-like shape, and, if filled from the bottom to that point, and left a little dishing, or, when worn out to that depth, it might well be said to represent the internal surface of a potash kettle; but, below this depth, it takes a slight winding turn, of the screw or snail-shell form.

To one conversant with similar phenomena, now in progress, on a less extended scale, along the bed of Roaring Brook, a small stream emptying into the Boquette, about half a mile below, and along the beds of other such streams, in all mountainous regions, the conviction can scarcely be resisted, that this immense cavity, as large as it is, and as hard as is the granite or feldsparthic rock in which it is formed, must have been wrought out by the sand and gravel, with, perhaps, some larger pebbles, set, and, for a great length of time, kept in a whirling motion by a very rapid current of water passing over the rock; and that this rock was once the bed, or bordering on the bed of the river.

What gives peculiar interest to this extraordinary development, is its great antiquity, an idea with which the mind, on reviewing all the facts connected with it, is unavoidably impressed.

1st. The agency by which this excavation was made, must have been, for a very long period of time, at least several hundreds, probably some thousands, of years in active operation. A very long period of *uninterrupted* action would have been necessary for accomplishing such a work. But other existing phenomena show that it must have been done, not by a continuous, active agency, but by an agency active only at high water intervals; and, to be convinced that such was, in this case, most probably the fact, we have only to reflect that a current above that of low water mark only, would supply the materials requisite for producing such an effect.

2d. Some little time must have been occupied in excavating the valley to the depth of eighty feet, perhaps; that is, to its present level, where the river now runs.

3d. Some centuries must have elapsed since the valley, thus excavated, has remained undisturbed; as will appear from an examination of very large trees, found on the intervals, when the valley was first settled.

4th. Lastly, we find that a period of time, of greater or less duration, must have passed away after the valley became thus lowered, and prior to the commencement of the recent timber growth, from the fact that, beneath the roots of the stumps and trees, now standing, are embedded, several feet under ground, entire large trees, which must have been buried while the soil was forming, from which the more recent forest sprung.

In view of these facts, the mind is impressed with an idea of the still greater antiquity of the drift formation, which must have been, as a little reflection will show, quite anterior to the commencement of these developments.

It will be proper to state, on closing these remarks, that the writer of this article, though familiar, in his earlier days, with the spot here attempted to be described, has visited it but once for many years, and then with no view to a scientific description of it; so that the facts here stated are taken wholly from memory, as derived from the impressions they have left on his own mind, and the statements he has received from Mr. Bishop, the very intelligent owner of the farm and mills above named. Yet they are, he has reason to believe, generally and substantially correct.

New York, August, 1848. ORSON KELLOGG.

The Secretary also read the following report by Mr. Kellogg, made in obedience to a former request of the Association of Geologists and Naturalists:—

Indian Antiquities.

Report of the Committee appointed at a Meeting of the Association of American Geologists and Naturalists, held in the City of New York, September, 1846, *to make examination of Phenomena in Essex County, New York, supposed to have some connexion with Indian Antiquities.*

To any one conversant with the history of the American Revolution, every suggestion having reference to Indian antiquities in the

neighbourhood of Ticonderoga and Crownpoint, and along the whole extent of Lake Champlain, introduces associations of peculiar interest; while to another, acquainted also with the truly classic character of the geology of Essex county, such associations become doubly interesting, where such antiquities and such geology are supposed to have any real connexion. But, when contemplating a field where the imagination of man has wrought so many magic changes; where colophonite and garnet have been converted into silver ore, and the granite rock, slightly coated with arsenic, into pure silver; where large beds of the scales of mica, from disintegrated granite, have been transformed into gold dust, and mined at great expense, and the sulphurets into massive gold; and where copper has been wrought from copperas ore, and numerous coal mines are still found in strata, side by side with granite rocks; the inquiry very naturally occurs, whether the imagination of man may not also have had its agency in creating such antiquities.

Under such impression, the writer of this article, in the month of August, 1846, with two intelligent men of Keeseville, and at their request, went to the spot where occur the phenomena in question, but was, from incredulity derived from considerations above suggested, so unfortunate as not to appreciate the views of all the amateurs of Indian antiquities in that vicinity. A few weeks afterwards, a brief oral description of the appearances about the place was made to the Association of Geologists, &c., then convened in New York, and a committee was appointed to make further investigation, and prepare, in writing, an appropriate report.

In discharge of the duties thus assigned, the member of said committee, by whom this hasty article is prepared, about the first of November of the same year, communicated with his associate at Burlington, Vt., and appointed the month of August, of the year succeeding, for the object above named. A few days previous to the time fixed on for the meeting, he wrote again, and received in reply, that his associate would be happy to make the excursion for the purpose contemplated, and had intended to do so up to that date, but should not then be able, owing to some unexpected engagements. Not caring to visit the spot again for his own gratification, he deferred the examination, and appointed, for the same purpose, August, of the present season. Again, a few days previous to the time appointed, a letter was despatched, but no reply has been returned, and we are bound to believe the letter failed of its due receipt.

In view of these circumstances, it has been deemed expedient to

draw up from memory, and present as a substitute for an authentic report, together with a suggestion or two, a brief statement of the facts originally elicited.

On the western shore of Lake Champlain, the face of the country is sometimes undulating, but generally hilly or mountainous, and frequently rocky. It is on the summit of one of these irregular eminences, a little more than a mile west of Port Douglas, and opposite to Burlington, in Vermont, that these supposed Indian antiquities are to be found. The top of this hill is probably from two to three hundred feet, as nearly as recollection of the surrounding scenery subserves the estimate, above the low grounds on the lake side, and perhaps half that height above the interval on the west. Across this summit, in a southwesterly and northeasterly direction, runs a dyke, as is also believed from recollection of its appearance, of Greenstone trap, and about three feet in breadth.

For the distance of some twenty feet, as nearly as is now recollected, the trap has been broken up, and removed to the depth of several inches, perhaps from six to ten; and over this stands the stump of a large tree, which appears evidently to have grown there since the dyke was thus disturbed. Very near this dyke, where thus broken, on the southerly side, are two cavities in the hard sienite rock, the surface of which is there bare, which appear to have been wrought by art, and used as mortars; the greater being about equal in size to a three-pail kettle, and the less about one-third as large. They are both quite regular in their form, and, the rock being a little sloping, the smaller is situated partially beneath the larger, and connected with it by a groove of a very few inches in length, which appears also to have been wrought in the same manner. On the opposite side, a little to the right, and a little farther from the dyke, where the earth has been removed to the depth of some ten or twelve inches for its discovery, is found another cavity of similar appearance.

In reference to these cavities, two inquiries have been agitated; first, as to their origin; and, secondly, referring that origin to the labour of human hands, as to the purpose for which they were wrought.

It has been suggested to the fruitful imaginations even of some very intelligent men of Essex county, that they must have been carved out at least some two or three hundred years since, as is evident from the subsequent growth of timber as above stated; that this must have been the work of the American Indian, from the fact that no other human being is supposed to have made footsteps there at a period so

remote; that it must have been done for the purpose of pounding up the trap thus broken, and washing out the silver or other precious metal hence derived; and that such metal does there exist, or at least did there exist, to reward and encourage the red man of the forest in his indefatigable toil.

In corroboration of this opinion, it is stated that a certain prophetess, known in that section of the country as sleeping Lucy, under the powerful influence of animal magnetism, retrograded three hundred years in the horology of her time-dial, actually witnessed the excavation, and saw by clairvoyant second sight, at a period thus remotely back, the work performed by a race of men, tall of stature and quite erect, of a copper colour, with high cheek bones and long black hair, and nearly or entirely naked; and that, this latter cavity not having yet been discovered, she instructed them to excavate the earth in a given direction, and at the distance of about half a dozen yards from the former, where they would find another similar to those already found, and where they actually dug and did find it.

For the degree of probability to be accredited to this species of evidence, and for the value of the mesmeric science in prosecuting such discoveries, we are referred to the testimony of the recent explorers on the Hudson, of the treasures of Captain Kid.

The geologist, however, would probably discover here the traces of an agency far more powerful than the arm of man, and marks of antiquity of much higher date than that of three hundred, or even three thousand years. The imagination can supply no natural causes now in operation, as in the case of similar phenomena along the beds of rapid streams in all mountainous regions, by which the appearances here developed could have been produced; and we are relieved from the difficulty of accounting for them, only by referring their origin to agencies at work before the hills were elevated, and long prior to the present order of things.

A query which spontaneously arises, as an inference derived from this view of the facts here presented, is, whether they are not to be regarded as evidence of the truth of that theory which should account for other marks, throughout all these northern regions, of both running and standing water in elevated situations, by supposing rather that the land has been since raised, than that the waters have been, as sometimes imagined, from any cause drawn off.

And while we regard these apparently artificial cavities as having a geological origin, though, during a period of great antiquity, similar to that of others in the vicinity of running water, and of a more recent

date, may we not grant to those who are so deeply interested in Indian antiquities, as the most rational way of accounting for the broken trap, and the appearances around the place, the probability that they were not wrought out by the red man of the forest, but formed at his hand, and used, perhaps without avail, yet for some such purpose as has been supposed?

ORSON KELLOGG.

New York, August, 1848.

On motion, *Resolved*, That the two communications of Mr. Kellogg be entered on the proceedings of the Association.

The Secretary, agreeably to a resolution of the Association on a former day, reported an alphabetical list of its members, which was ordered to be printed.

The Chairman of the Standing Committee reported, under instructions, the following resolutions, which were severally read, considered and adopted:—

1. *Resolved*, That our thanks are due to the officers of the Academy of Natural Sciences, for the use of their Hall, at the early part of the Convention's sitting.

2. *Resolved*, That the thanks of the Association be tendered to the Philadelphia Athenæum, to the Horticultural Society, and to the Franklin Institute, for the kind invitations addressed by them, severally, to the Association.

3. *Resolved*, That the thanks of the Association be presented to the General Secretary of the Association, and to the Secretaries of the Sections, for the faithful performance of the duties assigned them.

4. *Resolved*, That the thanks of the Association be presented to the President, W. C. Redfield, Esq., for the dignity and urbanity with which he has presided over its deliberations.

In acknowledgment of the last resolution, the President rose, and, in a few feeling and pertinent remarks, expressed his thanks for the compliment, and approval thereby conveyed, offering to one and all a reciprocation of the kind sentiments and wishes with which his sincere endeavours to subserve the cause of science, in the honourable post to which he had been called, had been so ably and successfully seconded by every member of the Association.

Prof. W. B. Rogers observed, that he was about to make a motion for adjournment, *sine die*, but, before doing so, he would bear witness to the general harmony which had pervaded their deliberations. The

co-operation of members, as a body, was in a spirit of peculiar unison, and he was happy to perceive this feeling manifested at this, the hour for dissolving their deliberations. It was the manner in which, he trusted, they would always associate. Science would, therefore, be advanced in an equal degree with that of the kindred feelings of their hearts.

He then moved that the Association now adjourn, *sine die*, which motion prevailed, and the meeting dissolved.

WALTER R. JOHNSON, *Sec'ry.*

Note.—The following is a corrected list of the Members, prepared and published agreeably to an order of the Association.

MEMBERS

OF THE

American Association for the Advancement of Science.

Note.—Names of deceased members are marked with an asterisk (*), and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals.

A.

Alexander, Prof. Stephen, Princeton, N. J.
Allen, Z., Esq., Providence, R. I.
Allen, Ira M., Esq., New York.
Ames, M. P., Esq., Springfield, Mass.
Alvord, Daniel W., Esq., Greenfield, Mass.
Ayres, William O., Esq., Boston.
Appleton, Nathan, Esq., Boston.
Alger, Francis, Esq., Boston.
Adams, Pentegast, Esq., Jefferson Co., N. Y.
Allen, J. L., Esq., New York.
Agassiz, Prof. Louis, Cambridge, Mass.
Andrews, Stephen P., Esq., New York.
Atlee, Dr. Washington L., Lancaster, Pa.
Adams, Solomon, Esq., Boston.
Aikin, Prof. W. E. A., Baltimore.
Alexander, John H., Esq., Baltimore.
Atkinson, Mr. —— ? Cumberland, Md.
Abbot, Dr. S. L., Boston.
Adams, Prof. C. B., Amherst, Mass.
Abert, Col. J. J., Washington, D. C.
Anthony, J. G., Esq., Cincinnati, Ohio.
Ashmead, Samuel, Esq., Philadelphia.
Adams, John G., Esq., New York.

B.

Brumby, Prof. Richard T., Tuscaloosa, Ala.
Burdett, F., Esq., Boston.
Bulfinch, Thomas, Esq., Boston.
Bache, Prof. Alex. D., Washington, D. C.
Bigelow, Dr. Jacob, Boston.
Beck, Dr. T. Romeyn, Albany, N. Y.
Bacon, President ——? Washington, D. C.
Baker, Eben., Esq., Charlestown, Mass.
Barratt, Dr. Joseph, Middletown, Ct.
Bacon, Dr. John, Jr., Boston.
Bradish, Hon. Luther, New York.
*Bomford, Col. George, Washington, D. C.
Bridges, Dr. Robert, Philadelphia.
Buchanan, Mr. R., Cincinnati, Ohio.
Belknap, George, Esq., Boston.
Beardsley, Dr. H. C., Painesville, Ohio.
Bond, Dr. Henry, Philadelphia.
Bakewell, Robert, Esq., New Haven, Ct.
Brown, Andrew, Esq., Natchez, Miss.
Blake, Eli W., Esq., New Haven, Ct.
Bell, Dr. John, Philadelphia.
Bolton, Richard, Esq., Pontotoc, Miss.
Bouvé, Thomas T., Esq., Boston.
Bonnycastle, Sir Charles, Montreal, Can.
Barber, Rev. Isaac R., Worcester, Mass.
Burgess, Rev. Ebenezer, Ahmednuggur, India.
Baird, Prof. Spencer F., Carlisle, Pa.
Bailey, Prof. J. W., West Point, N. Y.
Beck, Prof. Lewis C., New Brunswick, N. J.
Blake, John L., Esq., Boston.
Booth, Prof. James C., Philadelphia.
Briggs, Charles, Jr., Esq., Columbus, Ohio.
Browne, Peter A., Esq., Philadelphia.
Benedict, Prof. Farran, Burlington, Vt.
Brown, Dr. B. B., St. Louis, Mo.
Boyé, Dr. Martin H., Philadelphia.
Bache, Dr. Franklin, Philadelphia.
Blanding, Dr. Wm., Philadelphia.
Beck, Dr. C. F., Philadelphia.
Buckley, S. B., Esq., New York.

Budd, Dr. B. W., New York.
Browne, D. J., Esq., New York.
Barbour, J. R., Esq., Worcester, Mass.
Brown, Richard, Esq., Sydney, Cape Breton.
Brevoort, J. Carson, Esq., Brooklyn, N. Y.
Burnett, Waldo J., Boston, Mass.
Bigelow, Dr. Henry J., Boston.
Beadle, Dr. Edward L., New York.
Browne, Robert H., Esq., New York.
Boyd, Dr. George W., Charlottesville, Va.
Briggs, Charles C., Esq., Charlottesville, Va.
Bachman, Dr. John, Charleston, S. C.
*Binney, Dr. Amos, Boston.

C.

Coffin, Prof. John H. C., Washington, D. C.
Colman, Rev. Henry, Boston.
Clapp, Dr. Asahel, New Albany, In.
Channing, Wm. F., Esq., Boston.
Chace, Prof. George J., Providence, R. I.
Carpenter, Dr. Wm. M., New Orleans.
Carr, Dr. E. S., Castleton, Vt.
Cole, Thomas, Esq., Salem, Mass.
Cohen, Dr. J. J., Baltimore, Md.
Chilton, Dr. J. R., New York.
Chandler, John, Esq., Boston.
Cooke, George H., Esq., Troy, N. Y.
Cabot, Dr. Henry, Boston.
Coan, Rev. Titus, Hilo, Hawari.
Cleaveland, Prof. Parker, Brunswick, Me.
Couthouy, Joseph P., Esq., New York.
Clay, Joseph A., Esq., Philadelphia.
CONRAD, TIMOTHY A., Esq., Philadelphia.
Clingman, Hon. T. L., Asheville, N. C.
Coates, Dr. Reynell, Philadelphia.
Couper, J. Hamilton, Esq., Darien, Ga.
Cresson, J. C., Esq., Philadelphia.
Chauvenet, C. U., Esq., Annapolis, Md.
Coffin, Prof. James H., Easton, Pa.
Chickering, Dr. Jesse, Boston.
Campbell, Dr. A. B., Philadelphia.

Cooper, Wm., Esq., New York.
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L.

Lesley, J. P., Esq., Milton, Mass.
Lieber, Dr. Francis, Columbia, S. C.
Linck, Dr. Christian, Philadelphia.
Lovering, Prof. Joseph, Cambridge.

M.

Mantill, Reginald N., Esq., London, Eng.
Morris, Rev. R., Jackson, Miss.
Moss, Theodore F., Esq., Philadelphia.

S.

Seaman, Louis, Esq., Berlin, Prussia.
Sherwin, Thomas, Esq., Boston.
Smith, Dr. J. V. C., Boston.
Snell, Prof. E. S., Amherst, Mass.
Sparks, Pres. Jared, Cambridge.
Stimpson, Wm., Esq., Cambridge.

W.

Worcester, Dr. Joseph E., Cambridge.

ERRATA.

Page.	*line.*
103	5th from bottom; for "printing," read "pointing."
104	10th from top; for "into the actual error," read "with the actual error."
353	2d from top; for "Kirkland," read "Kirkdale."
353	7th from top; for "length," read "depth."
353	14th from bottom; for "some," read "most."
353	7th from bottom; After period, read "No bones were found below the Stalagmite."
354	9th from bottom; for "ten feet upwards," read "ten feet and upwards."
355	16th from top; in place of the sentence beginning "Of tortoises," read, "Of tortoises eight or ten different species are found there in great quantities."

OBJECTS AND RULES OF THE ASSOCIATION.

OBJECTS.

The Society shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction to scientific research in our country; and to procure for the labors of scientific men, increased facilities and a wider usefulness.

RULES.

MEMBERS.

RULE 1. Those persons, whose names have already been enrolled in the published proceedings of the Association, and all those who have been invited to attend the meetings, shall be considered members on subscribing to these rules.

RULE 2. Members of scientific societies, or learned bodies, having in view any of the objects of this Society, and publishing transactions, shall likewise be considered members on subscribing to these rules.

RULE 3. The Collegiate Professors of Natural History, Physics, Chemistry, Mathematics, and Political Economy, and of the theoretical and applied Sciences generally; also Civil Engineers and Architects who have been employed in the construction or superintendence of public works, may become members on subscribing to these rules.

RULE 4. Persons not embraced in the above provisions, may become members of the Association, upon nomination by the Standing Committee, and by a majority of the members present.

OFFICERS.

RULE 5. The Officers of the Association shall be a President, a Secretary, and a Treasurer, who shall be elected at each Annual Meeting, for the meeting of the ensuing year.

MEETINGS.

RULE 6. The Association shall meet annually, for one week or longer, the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be entrusted to the Officers and the Local Committee.

STANDING COMMITTEE.

RULE 7. There shall be a Standing Committee, to consist of the President, Secretary, and Treasurer of the Association, the Officers of the preceding year, the Chairman and Secretaries of the Sections, after these shall have been organized, and six other members present, who shall have attended any of the previous meetings, to be elected by ballot.

RULE 8. The Committee, whose duty it shall be to manage the general business of the Association, shall sit during the meeting, and at any time in the interval between it and the next meeting, as the interests of the Association may require. It shall also be the duty of this Committee to nominate the General Officers of the Association for the following year, and persons for admission to membership.

SECTIONS.

RULE 9. The Standing Committee shall organize the Society into Sections, permitting the number and scope of these Sections to vary, in conformity to the wishes and the scientific business of the Association.

RULE 10. It shall be the duty of the Standing Committee, if at any time two or more Sections, induced by a deficiency of scientific communications, or by other reasons, request to be united into one; or if at any time a single Section, overloaded with business, asks to be subdivided, to effect the change, and, generally, to readjust the subdivisions of the Association, whenever, upon due representation, it promises to expedite the proceedings, and advance the purposes of the meeting.

SECTIONAL COMMITTEES AND OFFICERS.

RULE 11. Each Section shall appoint its own Chairman and Secretary of the Meeting, and it shall likewise have a Standing Committee, of such size as the Section may prefer. The Secretaries of the Sections may appoint assistants, whenever, in the discharge of their duties, it becomes expedient.

RULE 12. It shall be the duty of the Standing Committee of each Section, assisted by the Chairman, to arrange and direct the proceedings in their Section, to ascertain what written and oral communications are offered, and for the better forwarding the business, to assign the order in which these communications shall appear, and the amount of time which each shall occupy; and it shall be the duty of the Chairman to enforce these decisions of the Committee.

These Sectional Committees shall likewise recommend subjects for systematic investigation, by members willing to undertake the researches, and present their results at the next Annual Meeting.

The Committees shall likewise recommend Reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent Annual Meetings.

REPORTS OF PROCEEDINGS.

RULE 13. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the Secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 14. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declares such to be his wish before presenting it to the Society.

GENERAL AND EVENING MEETINGS.

RULE 15. At least three evenings in the week shall be reserved for General Meetings of the Association, and the Standing Committee shall appoint these and any other General Meetings which the objects and interests of the Association may call for.

These General Meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections, and thus all the Sections may, for a longer or shorter time, reunite themselves to hear and consider any communications, or transact any business.

It shall be a part of the business of these General Meetings to receive the Address of the President of the last Annual Meeting, to hear such reports on scientific subjects as, from their general importance and interest, the Standing Committee shall select; also, to receive from the Chairmen of the Sections abstracts of the proceedings of their respective Sections, and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 16. The Association shall be organized by the President of the preceding Annual Meeting: the question of the most eligible distribution of the Society into Sections, shall then occupy the attention of the Association, when, a sufficient expression of opinion being procured, the meeting may adjourn, and the Standing Committee shall immediately proceed to divide the Association into Sections, and to allot to the Sections their general places of meeting. The Sections may then organize by electing their officers, and proceed to transact scientific and other business.

LOCAL COMMITTEE.

RULE 17. The Standing Committee shall appoint a Local Committee from among members residing at or near the place of meeting, for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements for the meeting.

SUBSCRIPTIONS.

RULE 18. The amount of the Annual Subscription of each member of the Association shall be one dollar, which shall entitle him to a copy of the proceedings of each meeting. The members attending an Annual Meeting shall pay, on registering their names, an additional assessment of —— dollars. These subscriptions to be received by the Treasurer or Secretary.

ACCOUNTS.

RULE 19. The Accounts of the Association shall be audited annually, by Auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 20. No Article of this Constitution shall be altered or amended without the concurrence of three fourths of the members present, nor unless notice of the proposed amendment or alteration shall have been given at the preceding Annual Meeting.

PROCEEDINGS.

First Day, August 14, 1849.

PURSUANT to a vote of the Association, the Second Annual Meeting was held in Harvard Hall, at Cambridge, Massachusetts, at 10 o'clock, August 14, 1849. The meeting was called to order by WILLIAM C. REDFIELD, Esq., Chairman of the preceding year, and the Secretary, Dr. JEFFRIES WYMAN, being absent, Prof. E. N. HORSFORD was requested to act as Secretary, *pro tem.*

The list of OFFICERS elected for the present year was then read as follows:

Prof. JOSEPH HENRY, *President.*
Dr. JEFFRIES WYMAN, *Secretary.*
Dr. ALFRED L. ELWYN, *Treasurer.*

STANDING COMMITTEE.

Prof. JOSEPH HENRY,
Prof. JEFFRIES WYMAN,
Dr. ALFRED L. ELWYN, } *Ex officio.*
WILLIAM C. REDFIELD, Esq.
Prof. WALTER R. JOHNSON.

LOCAL COMMITTEE.

JOHN A. LOWELL, Esq.
Dr. JACOB BIGELOW,
Hon. NATHAN APPLETON,
Hon. NATHAN HALE,
GEORGE B. EMERSON, Esq.
Prof. H. D. ROGERS,
Dr. A. A. GOULD,
of Boston.

Prof. LOUIS AGASSIZ,
Prof. BENJAMIN PEIRCE,
Lieut. CHARLES H. DAVIS,
Prof. E. N. HORSFORD,
Prof. ASA GRAY,
of Cambridge.

William C. Redfield, Esq., at this stage of the meeting, called to the Chair Prof. Joseph Henry, and as Dr. Wyman was not present, Prof. E. N. Horsford was unanimously elected, by ballot, to act as Secretary of the meeting. The Chairman nominated Dr. R. W. Gibbes and Dr. B. A. Gould, Jr., Assistant Secretaries.

Agreeably to the Seventh Rule of the constitution, the following gentlemen were elected upon the Standing Committee:

Prof. A. D. Bache, Prof. Louis Agassiz, Prof. Benjamin Peirce, Prof. Asa Gray, Prof. James Hall, Prof. Benjamin Silliman, Jr.

The organization of the Association having been effected, Prof. Henry offered the following remarks:

The primary object of this Association is the advancement of Science, or the promotion of the discovery of new truths. We are not assembled to exhibit our knowledge of other men's thoughts, but to present to each other for consideration and friendly discussion new truths, the results of original investigation; to inform each other of what we have done during the past year, and to ask assistance, advice, and coöperation; to elicit by intercourse and the collision of mind, new suggestions, to be proved or disproved during the next year. It is to be presumed, therefore, that the time of the Association will not in any case be occupied with mere popular expositions of well-known subjects.

Much of the value of these meetings is derived from the personal conversation of the members; and it is therefore desirable that the daily sessions should not be too much protracted. On this account, those who present memoirs, as well as those who join in the discussions, should study brevity.

The first paper was then read, by Dr. Gould, the author not being present, as follows:

Aurora Borealis. By Professor Secchi.

To Professor Henry, Secretary of the Smithsonian Institute.

Sir: — I saw a few days ago, in the newspapers, that you are engaged in collecting observations on the Aurora Borealis, to fix, if it be possible, the laws of this interesting phenomenon so little understood. When I was at Stonyhurst, in England, near Preston, about 70 miles N. E. from Liverpool, last October, I witnessed a display of this phenomenon, of extraordinary beauty, and remarked some

circumstances connected with it, such as are not commonly noticed. I think you will not be displeased if I trouble you with an account of them. My attention was engaged there, not only in consequence of the magnificence of the meteor, but also on account of my eagerness to witness it, never before having been fortunate enough to see any of remarkable beauty.

In the evening of October 17, 1848, at 6 o'clock, I was called out to see the Aurora, which grew already complete, as far as it appears commonly. Towards the north was an obscure circular segment, surrounded by a luminous ring about 10° or 12° wide, composed of luminous rays or beams perpendicular to the circumference of the circle. The top of the dark segment was about 40° high; at the points N. E. and N. W. there were two large luminous masses about 20° wide and 30° high, of a brilliant red purple color, striped with white and yellow beams; the most splendid of them was at N. E. Until now the effect was by no means extraordinary; when about ½ past 7 o'clock, another luminous mass appeared in the middle of the dark segment, and illuminated a large band of it from the horizon to the top of the arc in the neighborhood of the magnetic meridian. At the same time also a vermicular motion appeared all through the meteor, and scattered masses, detached from the ark, formed almost a beginning of another arc, but they very soon became curled into several irregular shapes. The southern part of the sky appeared extraordinarily dark, perhaps by contrast.

At half-past seven, several thin clouds appeared on the sky where the Aurora was shining, the arc began to grow faint, and after changing its form, almost vanished. I retired therefore into the house, thinking that all was over; but after an hour the clouds again appeared luminous towards the north, then disappeared, and the Aurora became quite wonderful. The dark segment was gradually shining, as if its matter had been set on fire; the two luminous masses situated at the N. E. and N. W. were greatly enlarged, grew higher, and united with that which was in the middle of the segment, a more vivid purple color remaining yet at their former place. Large masses of light, in the shape of immense waves and beams, shot forth from the arc, and lateral masses arose rapidly, passed the zenith to meet together and compose the crown. Before the crown was formed, several luminous waves appeared on the southern hemisphere, at an altitude of about 45°, rising gradually to meet with those coming from the northern side to form the crown. This fact which

was very apparent, and renewed several times during the continuance of the Aurora, shows that the luminous matter which appeared on the south was not the same which had already come from the north, and passed through the crown to the other side. Besides, a single glance at the crown showed that it was not made by the intersection of longitudinal beams, meeting together and intersecting one another; since the luminous matter was there disposed in extremely irregular figures and curls, and the motion of the waves, coming both from the south and north, was tending upwards to it.

We noticed carefully the position of the corona with respect to the stars which it covered, and comparing it on a globe conveniently disposed, we found them so far from zenith that lines drawn from them to us were nearly parallel to the axis of the dipping-needle at Stonyhurst. This measure could not be taken with greater accuracy, owing to the large extension of the crown, and its continuous agitation.

The most beautiful instant of the meteor was at ½ past 10. All the vault of the heaven was illuminated; long stripes, and beams of light red, white, and yellow, shooting from every point of the horizon, from S. E. all around through the north to the S. W., reached the crown. Beams suspended in the air, but not reaching the horizon, appeared also towards the south, leaving only a dark segment, nearly 45° high with a base of 90°, extended from S. E. to S. W. It is impossible to describe the effect and magnificence of this spectacle, which very seldom shines with all its greatness even in the northern countries. Many aged people who were accustomed to pay attention to the Aurora, assured me that it was quite extraordinary; and this was also the result from the register of meteorological observations kept very carefully at Stonyhurst observatory for several years by Mr. Weld. We could not find a more convenient object for comparison than that of an immense tent or canopy of transparent silk suspended in the inside of a golden dome, and extended in large folds and festoons from the crown down to the horizon, leaving only a large aperture towards the south. A gentle breeze shaking its folds, and sending to the eye the reflections of a more or less vivid light, can only represent the motion of the luminous waves which arose from the horizon towards the crown. The similarity of the motion produced was so close, that for some time I was in doubt whether I ought not to attribute it to the wind which at that time was blowing from N. W., where was now the most red and brilliant part of the meteor. I perceived,

however, very easily that this was not the case, because their progress was made both with and against the wind. But I could not satisfy myself on another doubt, namely, whether that motion was not at least indirectly produced by the wind, since it seemed that the most brilliant flashes of light coincided with the new impulses of the wind. This, therefore, deserves new observations.

The luminous waves were sufficiently large to extend themselves over several beams crossing them at right angles; they seemed to be composed not of any additional matter, but of that which by its motion and light formed the beams. These waves, as I shall continue to call them, began to move from the horizon. After rising several degrees, they stopped a little, and during the pause became very faint. After awhile they appeared rekindled and rose higher, until after several alternations of that kind, they reached the corona. In general, many of them were in motion together in the different parts of the sky, and their swift and irregular motion produced that liveliness which is the distinctive charm of this phenomenon, and without which it would scarcely be superior to the beauties of the dawn or sunset. In some moments of stillness, all the heavens appeared divided in stripes by the luminous beams, like the surface of a globe by the meridians, the pole being on the prolongation of the axis of the dipping-needle. After a few minutes of stillness, it paled gradually, and vanished in several parts, but only to give place to a new change of scene. Then the light revived suddenly all around with such rapidity, that it was impossible to resist the idea of an electric spark diffusing itself on the surface of an imperfect conductor or in vacuo. With this idea, I tried to explore the electricity of the atmosphere with a common atmospherical electrometer, whose point was unfortunately blunt, and I could not discover any sign of it. It is well known, however, that Count Morozzo, of Turin, obtained strong signs of electricity, during the shooting of the beams, with a very coarse electrometer, made of two straws, suspended with a thread at the top of a reed. This free construction of an electrometer, perhaps, is more suitable to this examination than the common one, where the straws are enclosed in a glass bottle. A galvanometer applied to an interruption made in a lightning-rod would be a yet better instrument in this case. I must also say, that when I examined it, the most vivid shooting light had disappeared. For want of good magnetic instruments, I could not see if there was any disturbance in the mag-

netic needles. But as the phenomenon was so large, it must have been observed somewhere.

The most brilliant part of the Aurora, at 8 o'clock, was at the N. E.; but after 10 it passed to N. W., so that it seemed to follow the apparent course of the sun. This very fact happens frequently, as I was then assured. The Aurora lasted until 4 o'clock in the next morning; and several times was renewed with very fine appearances, yet inferior to the first, although always extended almost over all the sky. In the following evening there was no Aurora at all, notwithstanding that the sky was perfectly cloudless and clear.

I paid a special attention to the colors of light displayed in the meteor. The most splendid were yellow, red, purple, and white; a yellow greenish color appeared in some places, but it was very faint, and evidently due to the contrast of the red with the blue of the sky finely illuminated with yellow. The colors, therefore, of the Aurora Borealis were the same with those of the morning Aurora in a pure atmosphere. Deep purple and violet color, like those which accompany the sunset, were wanting. This seems to lead to the conclusion, that no dense aqueous vapor takes place in the Aurora; since, in general, vapor displays more fiery colors than the pure air does. This conclusion is supported also by the observation of the stars. The large stars could be seen very distinctly, without haze around them; but the small ones could not be seen at all, on account of the brilliancy of the meteor, which was sometimes greater than that of the bright full moon. It will be better, in future, to observe the stars through powerful telescopes, to ascertain if they are perfectly without haze, and if they appear steady, or shaken by irregular refraction. This unsteadiness would prove that airs of different temperature are mixing together.

As a careful observation of the colors of the Aurora may lead to some interesting consequences, on account of the part which vapor may have in this phenomenon, and it is already difficult to define them accurately without a standard, I beg your permission to propose one which seems to me sufficiently good. This consists in the colors of Newton's thin plates. Nobili, observing them in his large chromatic scale, obtained by galvanic action on steel plates, observed that all the colors of the sky are contained in the two first rings. Those of the pure dawn extend themselves from the blue to the purple of the second order; when those of the clouds and vapors at the sunset are contained in the first, the fiery dies of the latter belong

totally to the first order.* These two orders are without green at all, although this color was enumerated, by Newton, in the second ring, by a mistake which is easily made, observing the rings in too narrow a scale. Several natural philosophers remarked the oversight of Newton, and I find that their observation is true; I can obtain rings whose second order is about an inch and a half in diameter, without any trace of green in it; and no green appears, when, by the diminution of pressure of the glasses, the central spot passes through all the colors of the same second order. This color is also wanting in the second ring obtained, with polarized light passed through the axis of a plate of calcar spar or topaz. These colors, being therefore constant and easy to obtain, may be used as a standard for comparison with those of the Aurora, and from them the action of aqueous vapor in it could be ascertained. If the absence of the fiery colors of the first order is constant, we can state that no dense vapor is found in the air where the Aurora shines, but only dry illuminated air.

I told you that the Aurora was interrupted by clouds, and reappeared at their vanishing. They were, in general, very faintly illuminated at the edge. It is a common observation, that very often the Aurora ends with clouds. On this fact, a very interesting question can be put: Are these clouds the effect of the Aurora, or do they prevent it when their formation begins? The solution of this question is closely connected with the explanation of the Aurora itself, which I shall not attempt to set forth, but I shall confine myself to point out this fact as one of those which any theory proposed must explain.

It seems now out of question that this phenomenon is electrical; but how this electricity is produced, is yet a mystery. Captain Beechey attributed it to electricity developed in the mixture of cold and warm air in northern countries. This is not the place to discuss the foundations of his theory. I shall only say that in this hypothesis we can explain several circumstances of the Aurora, and among them the influence of the clouds in it, and its disappearance when clouds appear. If the masses of air mixing together are overloaded with vapor, a part of it, after the mixture, will become condensed, and a cloud will appear. This being a better conductor of electricity than dry air, will easily discharge the electricity to the earth, and no luminous appearance will take place. This also might be the reason

* Nobili Opera, Tom. I. p. 170.

why, in foggy and damp seasons, the Aurora seldom appears, but generally only after a few days of fine weather. The faint light which accompanies the emission of steam, in hydroelectric machines, before the steam becomes visible vapor, might bestow an experimental proof of Captain Beechey's theory.

Those who attribute the origin of the Aurora to electricity are puzzled in finding the beams of light, which they regard as electric currents, perpendicular to the currents of the earth, instead of being parallel to them, as they ought to be according to Ampere's theory. Professor Pianciani considering that sometimes the beams are seen entwined with streams of light, thought they could be compared with electro-dynamic coils. But whatever may be the disposition of the electricity in the beams, I observe that they are neither perpendicular nor parallel to the currents of Ampere, but that each beam lies in a plane passing through the resultant of the terrestrial magnetic forces, that is, through the direction of the dipping-needle. The large arc which commonly constitutes the Aurora is perpendicular to this direction, but the positions of beams, of which it is composed, are in the said planes. The undulatory motion, which is propagated along the beams, excites the idea of luminous matter running from the points of the horizon towards a central line. That electrified matter should be so attracted by the earth towards the resultant of its magnetical forces, agrees well with what we know about the mutual action of magnetism and electricity. The electricity, however, during the travel towards the line of the said resultant, seems to be gradually absorbed by the earth, since the quantity visible near the horizon is extremely great in comparison with that which reaches the crown. According to this idea, the beams would not be prismatic, or cylindrical columns, or arches rising from the limits of the horizon, and passing above the head of the observer to meet with one another in the crown; they would only appear so, being really as many masses starting from the horizon, enveloping occasionally the observer himself, and converging towards the common line of attraction, as so many slices contained between planes intersecting one another in the line of the dipping-needle, through which planes the matter is attracted by the earth from any point of the atmosphere, and tends to gather in the direction of the attracting magnetical force.

But, sir, I perceive that I am wandering in the fields of hypothesis more than a simple communication would require. I shall not, however, apologize for it, being aware that it happens very often that an idle

speculation provokes an useful observation, which might otherwise have been overlooked.

I remain, therefore, respectfully, your obedient servant,

A. SECCHI,
Prof. of Math. and Nat. Phil.

Georgetown College, D. C., April 12, 1849.

P. S. This letter was almost finished, when I happened to see in Silliman's Journal of last month, p. 127, that the same Aurora was seen also in America, at New Haven; this encouraged me to send you further details, that they may be useful to compare both appearances, in case that observations have been made.

After the reading of the above communication, Dr. HARE made the following remarks:

The idea is suggested, that moist air is a conductor. I am enabled to say, from experimental observation, that moist air is not a conductor; it only imparts conducting power to the surface of glass or other insulators, by its deposition on them, and thus paralyzes apparatus which for its efficiency requires insulation. From the conductor of an electrical machine, I so suspended an iron rod, terminated by a knob, as to descend through the open neck of a bell glass placed over a capsule of water sufficiently hot to yield a copious cloud of steam. Under these circumstances, the red hot iron being incapable of being coated with moisture, the sparks proceeded from it as well as if the condensed vapor or fog had not been present.

Professor Faraday has inferred — and no man's experimental inferences are more worthy of confidence — that the torrent of electricity produced by a jet of high steam is caused by the friction of the aqueous globules. Whether from this cause, or a change of capacity resulting from condensation, there is evidently in nature an enormous power of electrifying those immense masses of condensed water, of which thunder clouds are formed, so as to create the gigantic discharges which take place during electrical storms. Were moist air a conductor, these clouds could not retain their charges until intense enough to give the immense sparks, which are called lightning; nor could a spark be given by a jet of high steam, since the moisture which it so copiously supplies, would carry off the electricity, imperceptibly, as soon as generated.

In respect to the Aurora Borealis, I will advert to a suggestion which I published in 1836,* agreeably to which, there are three great

* See Silliman's Journal for that and the following year.

concentric spaces: first, the earth, which is a conductor; then the atmosphere, which is a non-conductor, even when moist; lastly, the space beyond the earth, which must act as a conductor, as we know that a certain degree of rarefaction makes air a conductor, and that electricity may be discharged through the cavity of an exhausted tube, uninfluenced by its length.

We have, then, in this great globe, the denser portion of its atmosphere, and the rarer medium beyond, all the features of the Leyden jar, and, of course, an enormous capability of receiving the opposite states requisite to the production of electric discharges. That the faculty of producing those states exists, is clear from the considerations above-mentioned. It is, then, evident that the intermediate conducting concentrated sphere, constituted by the atmosphere, is as competent to receive a charge of electricity as a hollow glass jar or globe; and that there are in nature commensurate means of electrical excitement; and it is reasonable to assume that the intermediate atmospheric sphere to which the others serve as conducting coatings, may have its inner and outer surfaces oppositely charged; and that in consequence discharges may ensue either disruptively, as lightning, or convectively, as in tornadoes and hurricanes, which are conceived to owe their centripetal blasts to the ascent at the axis of the electrified particles repelled by the similarly electrified terrestrial superficies, and attracted by the outer and oppositely electrified atmospheric surface, the buoyancy of which, arising from the rarity due to its altitude, prevents it from descending. Moreover, there may be diffuse Aurora-like discharges from one part of the rarer external surface of the atmospheric sphere to another, which may even take place between the arctic region and the antarctic. All the water within the arctic and antarctic circles, converted by extreme cold into snow or ice, must be rendered as perfectly non-conducting as glass, and when in that state must be highly qualified for the reception and retention of electricity, until a sufficient intensity is attained to effect discharges through the rare medium existing at more than thirty miles above the earth's surface, where the air is as rare as it can be rendered by the most perfect air-pump, and, consequently, as above-mentioned, as fully competent to act as a conductor.

I shall not now expatiate respecting those theoretic suggestions, already published, agreeably to which the opposite electricities are to be ascribed to opposite polarities imparted to different portions of an ether which pervades the creation, and of which the undulatory affec-

tions are now generally supposed to be the cause of light. I will postpone that topic to a more suitable occasion.

That the splendid and interesting phenomena described in the paper which has been read are attributable to electricity, I have no doubt. That they are uninfluenced by the thermo-electric currents to which terrestrial magnetism is ascribed consistently with the Amperian theory, is not inconsistent with their electrical origin. Currents of this kind, as those produced by galvanic or voltaic batteries, have no reaction with diffuse discharges of frictional electricity, whether produced by a machine or aqueous vapor. It is only in an abnormal state of extreme concentration that discharges of this nature have any inductive reaction with those on which magnetism is depended.

Professor HENRY. — The paper of Professor Secchi seems to me to be one of considerable interest. It contains a number of ingenious suggestions, which may lead to new results. One fact alluded to in this paper is highly important, and though taken for granted, since the days of Franklin, has only lately been fully established. I allude to the connection of the Aurora with electricity. Besides the observation mentioned in the preceding paper, I am informed by Mr. Herrick, of New Haven, that an electrical action had been observed at that place on the wires of the telegraph at the time of the appearance of the Aurora. The same fact has also been observed in England and on the continent, during the last year. It is highly desirable to ascertain whether this action is one of actual transfer of electricity from the space at one end of the wire to that at the other, or whether it is an inductive action of the Aurora at a distance, disturbing for an instant the electrical equilibrium of the wire. This could be readily determined by the character of the action on the needle of a galvanometer.

There was an Aurora last night visible at this place, which exhibited some peculiarities not frequently observed, so far as I am informed, in this latitude. These were pointed out to me by Dr. A. D. Bache, and are similar in a degree to the appearances observed in Siberia. The Aurora, in these high latitudes, frequently presents the appearance of a number of concentric scrolls or curtains, the general axis of which is parallel to the dipping-needle. The Aurora of last night consisted, while we were observing it, of a number of parallel beams which together formed the skeleton of an arch with an irregular curtain border at the lower edge.

I may mention to the Association that the Smithsonian Institution, in connection with an extended system of meteorology which it has undertaken to establish, has issued directions for observations of the Aurora. These directions are similar to a set issued by the directors of the observatory at Toronto, for observers in Canada. The observations made in the two countries will thus form one extended system. The proprietors of the several telegraph lines have offered to grant us the use of their wires for meteorological purposes, and it is hoped when the lines are completed, and we have established a set of observers, extending, for example, from Toronto to Washington, or even farther South, we shall be able to study the phenomenon of the Aurora with more precision than it has ever been studied. On a long line extending north and south, the observer, for example, at Toronto, having noticed an Aurora, may call the attention to it of all the observers along the line, and thus the extent of the visibility, and the simultaneous appearance of any peculiar phase of the meteor, may be readily determined.

The second paper was read by Professor GRAY, as follows:

THE POLAR PLANT, OR SILPHIUM LACINIATUM. BY BENJAMIN ALVORD, Brev. Maj. U. S. Army.

To the Presiding Officer of "the American Association for the Promotion of Science," Cambridge, Massachusetts.

I propose to offer a few observations which may serve to excite attention to the *Polar Plant* of the western prairies, or the *Silphium laciniatum.*

In the "*Manual of the Botany of the Northern United States,* by Asa Gray, M. D." published in 1848, I notice that he says, p. 219, under the head of *Silphium laciniatum,* "Lower leaves said to present their faces uniformly north and south (which is not the case,) and thence called Compass Plant."

Many may recollect that in August, 1842, and in January, 1843, I made communications to the National Institute, at Washington, which were published, setting forth the peculiarities of this plant. But I made known the polarity of the leaves of the plant, not as a discovery of my own, (though it was then unknown to the scientific world,) but as a fact well known and notorious among hunters, trappers, officers of dragoons, and others who traversed the prairies. The fact being disputed by such respectable authority as that of Dr. Gray, it has

become a matter of scientific interest, as well as of justice to myself, that I should sustain my former testimony in the case.

But in the outset, it is proper first to notice a mistake in the description given in the above extract. It says that "the leaves are said to present their faces uniformly north and south." It should have said, that it is alleged that the radical leaf presents its faces uniformly to the east or west, the plane of the leaf being north and south, or coinciding with the meridian plane. This is uniformly true of the radical leaf of the *Silphium laciniatum*, when there are no disturbing causes. I have had, myself, opportunities to notice it on the prairies, in various portions of the west, for four years. In the vallies, or lower portions of the rolling prairies, where most sheltered from the winds, this polarity is most accurate, and the plants are seen arranged all parallel to each other. This is true of the radical leaf from one to two feet in height, before it grows up, as it does in the second year, to the flowering plant.

But my principal object in making this communication, was to offer the following testimony on this topic. In answer to communications written before I had seen the work of Dr. Gray, in which I especially made inquiries as to how far west those officers had seen the plant, I received the following replies: —

Surgeon S. G. I. De Camp, one of the most respectable and experienced officers in the medical department of the army, who accompanied General Kearney's expedition to New Mexico, writes me as follows, from Carlisle Barracks, Penn., March 20th, 1849: "I have seen the Polar Plant growing luxuriantly from Fort Towson, near Red River, to Fort Leavenworth, on the Missouri. In the direction of Santa Fé I have not observed it more than two hundred miles from Fort Leavenworth, that being about the point where the rich prairie lands terminate, and what are called "the plains" commence. In the direction of the south pass of the Rocky Mountains, it is found as far west as the Pawnee villages, on the *bottoms* of the Platte River, a little below Grand Island, say 300 miles from Fort Leavenworth. I have been well acquainted with the peculiarities of the Polar Plant for nearly thirty-two years; and am perfectly satisfied that while the plant is healthy and of a vigorous growth, the edges of the leaves point northerly and southerly with sufficient accuracy to be of great service to the bewildered traveller."

Brevet Major Henry S. Turner, then of the first regiment of dragoons, who accompanied General Kearney's expedition to New

Mexico and also to *California*, and has since resigned his commission, writes me as follows, under date of St. Louis, April 22d, 1849: "I have no recollection of having seen the Polar Plant in New Mexico or California; indeed I may say, positively, that it was nowhere upon our route, either going to, or returning from California, west of the rich prairies which border upon our frontier. With respect to its property of polarity, or pointing north and south, my attention was first drawn to it, fourteen years ago, by Major Nathan Boone,* of the first dragoons, perhaps the oldest, the ablest, and the most experienced pioneer of the west. In all my subsequent prairie expeditions, I have uniformly observed this property in the Polar Plant, and have never observed a departure from it, except when there was some assignable cause apparent to interfere with the natural direction of its growth, such as winds, the trampling of buffalo or cattle, &c."

I trust that the testimony of these accomplished officers will be regarded as conclusive as to the facts. At the west the polarity of this plant is so notorious that any such testimony would be pronounced idle and unnecessary, it being considered as a settled and household fact. An amusing example of this occurred in 1842. A country paper in Missouri, alluding to the publication of my first letter on the topic, the exact language of which had not been noted, sneered at the idea of any one claiming a "discovery" of such a plant, as it was one they had always known, and an every-day affair in that country. The editor had not noticed that I only proposed to commudicate to a scientific institution knowledge of a plant, "well known" at the west, but which had not before been brought to the attention of men of science.

Subsequent to the publication of my own, Dr. Gideon B. Smith, of Baltimore, sent a communication, Aug. 29, 1842, to the National Institute, not calling in question the fact of the polarity of the plant, but averring that he had published a description of such a plant in his American Farmer for 1833. Nevertheless, the most eminent profes-

* This respectable officer, now advanced in years, and who came late in life into the dragoon regiment, is a son of the celebrated Daniel Boone. A venerable gentleman in Kentucky, who had been engaged in the fur trade in his early life, told me that Daniel Boone once said to him, that "his son *Nathan* was a *better woodsman* than himself, though he claimed the superiority as a hunter and Indian fighter." I might add in this place that some aged Indians west of Arkansas, who were questioned on the subject, said that they had often made use of this plant as a guide in cloudy weather, and that its peculiarity was well known to the Indians. B. A.

sors of Natural Science in the country were ignorant of the existence of any such property in the *Silphium laciniatum* at the date of my publication. Dr. Smith added the interesting fact that another Silphium, the *Silphium terebinthinaceum* has leaves alternately disposed north and south, and east and west. Soon after, I think in September, 1842, a communication was made to the National Intelligencer, from Indiana, stating that not only this plant, but others on the prairies were found to possess a similar property. This leads me to add, that a distinguished engineer in Louisiana informed me, in 1848, that since his attention had been drawn to this subject by me, he had noticed several other plants in which he thought the leaves were attracted towards the north.

My own observation of the flowering plant of the *Silphium laciniatum*, (and it grows to this I think the second year,) has led me to suppose that its leaves exhibit the same polarity as the radical leaf, but, in the former case, the leaf takes a medium position between its tendency to dispose itself symmetrically in reference to the stalk, and its tendency to point towards the north. Of course many disturbing causes are liable to operate to affect this result.

The cause of the polarity of the *Silphium laciniatum*, is yet to be discovered. A careful analysis, by the use of the most delicate tests, gave no trace in it of the existence of the magnetic oxide of iron or of any other compound of iron. I have conjectured, therefore, that the polarity may be due to electrical currents, as the plant is full of resinous matter, and is sometimes called the "*Rosin Weed.*"

The above abstract embraces the principal facts yet known; but the naturalist will see that much remains to be done in the way of accurate and judicious observation. The allusion to other plants is made, as it is worthy of investigation, and may lead to some interesting generalizations. Proper observation and experiments may discover traces of some general law for these results.

The removals incident to my profession, have always interrupted any series of observations I had commenced. The plant is found in southern Michigan, but I have not yet discovered it in this portion of the state. It is found on rich prairies, and its indications are most to be relied on in midsummer, before any decay has occurred. I could not find it on the barren prairies of western Texas, between the Neuces and the Rio Grande, or anywhere in Mexico; but I have seen or heard of it on the prairies from Texas to Iowa, and as far west as about three hundred miles from the western boundary lines of the states of Arkansas and Missouri.

The above is offered, to be submitted, if you think proper, to the respectable body over which you preside; and I would also request that you give it to Professor Silliman for publication. I suppose he will attend your Convention, and I promised him some years since to send a communication on this topic.

Very respectfully, your obedient servant,

BENJAMIN ALVORD, Brev. Maj. U. S. A.

Fort Gratiot, Michigan, July 28, 1849.

Prof. HARE, inquired whether any effort had been made to test the polarity of this plant by any artificial power. If the plant was sensitive to the magnetic influence, of course it ought to obey the more concentrated influence of a powerful steel magnet, or an electro-magnetic magnet. If it should be found insusceptible to such influences, it could not be supposed to obey the less powerful influence of the earth.

Prof. GRAY. I have another communication which has been entrusted to me to present, upon the plants of Wisconsin, and in that paper, I have just found a note upon the subject, showing that this fact is generally assumed at the west; that there is something about it which has led to that inference, and that it is a matter of general belief among most intelligent men throughout that whole region. The paper is by I. A. Lapham, Esq.; and I can vouch for his being a very respectable, and I think a very competent observer. The note is as follows: —

"The large radical leaves of this species of the sunflower tribe, when growing in tufts or bunches on the dry open prairies, rise so much above the grassy turf as to form conspicuous objects; and when thus exposed, they generally present their flat surfaces towards the rising and setting sun — thus turning their numerous pointed lobes towards the north and south. Hence it is called the "Compass plant," and is useful as a guide across the prairies."

I would state for the benefit of gentlemen of the Association who may be curious to determine the general fact, that two specimens of the plant are growing in the Botanical Garden. One of them is in the flowering state, but still holds its radical leaves. The other does not flower this year, and has merely its radical leaves, and is therefore in the condition which is said to present the most certain indications. I hope gentlemen will examine them. I think I received both plants from the gentleman whose communication I now hold in my hand,

[I. A. Lapham.] I would state that the other species, mentioned in the communication as having its leaves directed alternately north and south, and east and west, is also growing in the Botanical Garden, namely the *Silphium terebinthinaceum*. But its leaves are chiefly radical; the stem-leaves are few and small, and evidently are not arranged in the manner stated. Probably the plant intended is another species still, and a common one, the *Silphium perfoliatum*, which is also cultivated in the Botanic Garden, and may be found in many gardens in Cambridge. This has large opposite leaves, united in pairs across the stalk, the successive pairs crossing each other at right angles, according to the normal mode in opposite-leaved plants; so that if it happens that any one pair is directed north and south, the second will be east and west, and so on. It would be strange if the leaves should not stand sometimes pretty nearly in this position. But the plants in the garden quite as frequently present their leaves in the intermediate directions. As to the "polar plant," the testimony appears to show that the popular belief has some kind of foundation; and I suppose that Mr. Lapham, referring the phenomenon to the disposition of the surface of the leaf with respect to the light, has stated the case better than the gentleman who made the original communication. It is well known that leaves ordinarily turn their upper surface to the light; but vertical leaves, as these incline to be, tend to take a position which exposes the two surfaces equally to the light of the sun; and such upright radical leaves, by presenting their surfaces to the east and west, most nearly fulfil this condition. The idea of "polarity" could only mislead; and as to "electrical currents" induced by the resin the plant contains, it would be seen that they could not exist in a resinous body; resin being a non-conductor of electricity.

Dr. Gray proceeded to say, that however the case might be with respect to the plant on the prairies, the President could, he thought, vouch for the fact that those growing in the Botanic Garden did not present the edges of their leaves to the north and south, or in any one plane more than another. The brief statement in the systematic work referred to by Major Alvord, was based on these plants, which at that time, as now, did not exhibit the peculiarity in question.

Dr. Hare alluded to the common belief that the sunflower turns towards the sun, and inquired whether there was any truth in it. He had looked at the plant to see whether it had any such tendency, but

would confess that his own observation had not led him to believe that it did turn to the sun. If such notions could become popular, without good ground in the one case, they might also in the other. He would admit that he was not so well qualified to judge of the facts where the sun, as where magnetism was concerned.

Prof. GRAY. The fact alluded to has found its way into poetry, and out of the domain of science; and is now regarded, in scientific works, as a popular fallacy. The heavy sunflower stands in unstable equilibrium on its stalk, and is liable to nod by its own weight. Doubtless it is more apt to droop towards the sun than in any other direction, simply on account of the sun's action on a sultry day promoting the exhalation from the side of the stalk on which it shines, wilting it, as it were. But that it follows the sun in its diurnal course is not believed to be the fact.

Prof. HENRY, in reply to a question, remarked that the leaves of the plants of *Silphium* in the Botanic Garden, certainly do not point to the north and south. As to the plant in the prairies, respecting which such testimony was adduced, it was a subject worthy of careful and precise investigation.

Rev. Mr. MORRIS (of Jackson, Mississippi). In surveying upon the prairies, for several years, I have observed that in running compass lines, north and south, the edge of the leaf was seen, so that the plant was not at all conspicuous; but in running lines east and west, the whole plant was seen, and it was a very conspicuous object.

Prof. PEIRCE inquired whether Mr. Morris had observed these leaves to conform to the variation of the compass or not.

Prof. MORRIS. The variation in Illinois, where I spent some years, is but 7° or 8°, and was not observable. The leaf did not show the variation, at least so far as I recollect now; for it was twelve or fifteen years ago. But the leading fact was one which we were called upon to observe frequently, that in running a north and south line, we could scarcely see the leaf; but in running east and west, the whole plant was very conspicuous.

The third paper was offered by Prof. GRAY, as follows: —

PLANTS OF WISCONSIN. BY I. A. LAPHAM, ESQ.

To the American Association for the Promotion of Science:

Wishing to contribute to the objects of your Association, I send the following enumeration of the plants found in this new State, so far as they have yet been observed. It is, doubtless, very far from being complete; many species remaining to reward future explorers.

The vicinity of the "Great Lakes," Superior and Michigan; the elevated plateau between Lake Superior and the Mississippi river; the "pineries;" the heavily timbered land; the "oak openings," and the prairies, may each be considered as distinct botanical districts, within this State, affording plants peculiar to themselves, and giving great richness and variety to our flora.

Mr. THOMAS NUTTALL was the first botanist, so far as I can learn, who visited Wisconsin. He passed Green Bay by the portage of the Neenah (Fox) and Wisconsin rivers, to Prairie du Chien, and thence down the Mississippi, as early as about the year 1813. In his very valuable "Genera of North American Plants," published in 1818, he makes frequent reference to localities in this State, and has described thirteen new species first discovered by him in these regions.

The next notice of our plants was published in 1821, in Silliman's Journal,* by Prof. D. B. DOUGLASS and Dr. JOHN TORREY; being "a notice of the plants collected by Prof. Douglass, in an expedition under Governor CASS, during the summer of 1820, around the Great Lakes and upper waters of the Mississippi." One hundred and ten plants are enumerated, many of them from within the limits of this State; and three are indicated as new species.

In 1823, Major LONG, with a party of scientific gentlemen, under the direction of the Secretary of War, traversed the North West Territory (as Wisconsin was then called); but unfortunately the botanist was detained, and did not join the expedition. We have, consequently, only an account of a few plants gathered by the late lamented THOMAS SAY, naturalist to the expedition; these were examined by LEWIS DE SCHWEINITZ, an accomplished botanist of Pennsylvania, and a list of them published in the Narrative of the expedition.

The next and last published notice of our plants is in Schoolcraft's "Narrative of an Expedition through the Upper Mississippi to Itasca Lake, the actual source of that river, in 1832." This expedition was accompanied by the late Dr. DOUGLASS HOUGHTON, whose premature

* Vol. IV. p. 56.

death in Lake Superior, while performing his arduous duties of State Geologist of Michigan, is sincerely regretted, not only by all who knew him, but by all the friends of science. The list of plants collected by him in this expedition, numbers two hundred and forty-seven, of which eight were new and undescribed.

Numerous prepared specimens of Wisconsin plants have, within the last few years, been distributed among the botanists of our own and other countries; and the critical notices kindly returned by them, have been of much assistance in making this enumeration. It embraces one hundred and thirty of the natural orders or families, four hundred and sixteen genera, and eight hundred and forty-nine species — all found within thirty miles of the city of Milwaukie, unless other localities are mentioned.

RANUNCULACEÆ. (The Crowfoot Family.)

Clematis, *Linn.* (Virgin's Bower.)
  Virginiana, *Linn.* (Common Virgin's Bower.)
Pulsatilla, *Tourn.* (Pasque-flower.)
  patens, *Mill.* (Anemone patens, *Linn.*)
Anemone, *Linn.* (Wind-flower.)
  nemorosa, *Linn.* (Low Wind-flower.)
  Virginiana, *Linn.* (Fall Anemone.)
  multifida, *DC.* Shore of Lake Superior. *Dr. Z. Pitcher.*
  Pennsylvanica, *Linn.*
Hepatica, *Dillenius.* (Liver-leaf.)
  triloba, *Chaix.* (Round-leaved Hepatica.)
  acutiloba, *DC.* (Sharp-leaved Hepatica.)
Thalictrum, *Linn.* (Meadow Rue.)
  anemonoides, *Michx.*
  dioicum, *Linn.*
  Cornuti, *Linn.* (Meadow Rue.)
Ranunculus, *Linn.* (Crowfoot.)
  aquatilis, *Linn.* (White Water-Crowfoot.)
  Purshii, *Richards.* (Yellow Water-Crowfoot.)
  rhomboideus, *Goldie.*
  abortivus, *Linn.* (Small-flowered Crowfoot.)
  recurvatus, *Poir.*
  Pennsylvanicus, *Linn.* (Bristly Crowfoot.)
  fascicularis, *Muhl.*

(RANUNCULUS.)
repens, *Linn.* (Creeping Crowfoot.)
Marylandicus, *Poir.* (Hairy Crowfoot.)
acris, *Linn.* (Buttercups.)
ISOPYRUM, *Linn.* (Enemion, *Raf.*)
biternatum, *Torrey & Gray.*
CALTHA, *Linn.* (Marsh Marigold.)
palustris, *Linn.* (Cowslip.)
COPTIS, *Salisb.* (Goldthread.)
trifolia, *Salisb.*
AQUILEGIA, *Linn.* (Columbine.)
Canadensis, *Linn.* (Wild Columbine.)
DELPHINIUM, *Linn.* (Larkspur.)
azureum, *Michx.* Upper Mississippi, *Dr. Houghton.*
HYDRASTIS, *Linn.* (Orange-root.)
Canadensis, *Linn.*
ACTÆA, *Linn.* (Cohosh.)
rubra, *Bigel.* (Red Cohosh.)
alba, *Bigel.* (White Cohosh.)

MENISPERMACEÆ. (THE MOONSEED FAMILY.)

MENISPERMUM, *Linn.* (Moonseed.)
Canadense, *Linn.*

BERBERIDACEÆ. (THE BARBERRY FAMILY.)

LEONTICE, *Linn.* (Caulophyllum, *Michx.*)
thalictroides, *Linn.* (Blue Cohosh.)
JEFFERSONIA, *Barton.* (Twin-leaf.)
diphylla, *Pers.*
PODOPHYLLUM, *Linn.* (May Apple.)
peltatum, *Linn.*

CABOMBACEÆ. (THE WATER-SHIELD FAMILY.)

BRASENIA, *Schreber.* (Hydropeltis, *Michx.*)
peltata, *Pursh.* (Water-shield.)

NELUMBIACEÆ. (THE NELUMBO FAMILY.)

NELUMBIUM, *Juss.* (Sacred Bean.)
luteum, *Willd.* Upper Mississippi, *Dr. Houghton.*

NYMPHÆACEÆ. (The Water-Lily Family.)

Nymphæa, *Tourn.*
odorata, *Ait.* (White Water-Lily.)
Nuphar, *Smith.*
advena, *Ait.* (Yellow Water-Lily.)

SARRACENIACEÆ. (The Pitcher Plant Family.)

Sarracenia, *Linn.*
purpurea, *Linn.* (Side-Saddle Flower.)

PAPAVERACEÆ. (The Poppy Family.)

Sanguinaria, *Dill.* (Bloodroot.)
Canadensis, *Linn.*

FUMARIACEÆ. (The Fumitory Family.)

Dicentra, *Bork.*
cucullaria, *DC.* (Breeches Flower.)
Canadensis, *DC.* (Squirrel Corn.)
Corydalis, *Linn.*
aurea, *Willd.* Upper Mississippi, *Dr. Houghton.*
glauca, *Pursh.* Blue Mounds.

CRUCIFERÆ.

Nasturtium, *R. Brown.*
palustre, *DC.*
natans, *DC.*
Cardamine, *Linn.*
rhomboidea, *DC.* (Spring Cress.)
hirsuta, *Linn.*
pratensis, *Linn.*
Dentaria, *Linn.* (Toothwort.)
laciniata, *Muhl.*
Arabis, *Linn.*
petræa, *Lam.* Shore of Lake Superior, *Dr. Z. Pitcher.*
lyrata, *Linn.*
hirsuta, *Scop.*
lævigata, *DC.*

(ARABIS.)

Canadensis, *Linn*. (Sicklepod.)

TURRITIS, *Dill*. (Tower Mustard.)

glabra, *Linn*. Shore of Lake Superior, *Dr. Z. Pitcher*.

stricta, *Graham*. Lake Superior, *Dr. A. Gray*.

brachycarpa, *Torr. & Gray*. Shore of Lake Superor, *Dr. Z. Pitcher*.

SISYMBRIUM, *Linn*. (Hedge Mustard.)

canescens, *Nutt*.

SINAPIS, *Tourn*. (Mustard.)

arvensis, *Linn*.

nigra, *Linn*.

DRABA, *Linn*. (Whitlow Grass.)

Caroliniana, *Walt*. Near Waukesha, *Mr. G. W. Cornwall!*

CAMELINA, *Crantz*.

sativa, *Crantz*.

LEPIDIUM, *Linn*. (Pepperwort.)

Virginicum, *Linn*.

CAPSELLA, *Vent*. (Shepherd's Purse.)

Bursa-pastoris, *Mœnch*.

CAKILE, *Tourn*. (Sea-Rocket.)

Americana, *Nutt*.

CAPPARIDACEÆ. (THE CAPER FAMILY.)

POLANSIA, *Raf*.

graveolens, *Raf*. Near Beloit, *Mr. T. McEl Henny!*

VIOLACEÆ. (THE VIOLET FAMILY.)

VIOLA, *Linn*. (Violet.)

blanda, *Willd*.

cucullata, *Ait*. (Blue Violet.)

pedata, *Linn*.

Muhlenbergii, *Torrey*.

pubescens, *Ait*. (Yellow Violet.)

CISTACEÆ. (THE ROCK-ROSE FAMILY.)

HELIANTHEMUM, *Trum*. (Rock Rose.)

Canadense, *Michx*.

HUDSONIA, *Linn*.

tomentosa, *Nutt*. Lake Superior, *Dr. Houghton*.

LECHEA, *Linn.* (Pinweed.)
minor, *Lam.* Upper Mississippi, *Dr. Houghton.*

DROSERACEÆ. (THE SUNDEW FAMILY.)

DROSERA, *Linn.* (Sundew.)
rotundifolia, *Linn.*
longifolia, *Linn.* L. Sup. to Upper Miss. *Dr. Houghton.*
linearis, *Goldie.* La Pointe, L. Superior, *Dr. Houghton !*
PARNASSIA, *Tourn.*
Caroliniana, *Michx.*
palustris, *Linn.* South shore of L. Superior, *Dr. Z. Pitcher.*

HYPERICACEÆ. (THE ST. JOHN'S-WORT FAMILY.)

HYPERICUM, *Linn.* (St. John's-Wort.)
pyramidatum, *Ait.* Waukesha, *Mr. G. H. Cornwall.*
Canadensis, *Linn.* Lake Superior, *Dr. Houghton.*
ELODEA, *Adans.*
Virginica, *Nutt.*

CARYOPHYLLACEÆ. (THE PINK FAMILY.)

§ 1. *Sileneæ.*

SAPONARIA, *Linn.* (Soapwort.)
Vaccaria, *Linn.* (Cow-Herb.)
SILENE, *Linn.* (Catchfly.)
stellata, *Ait.* (Starry Campion.)
Antirrhina, *Linn.*
noctiflora, *Linn.*
LYCHNIS, *Tourn.* (Cockle.)
githago, *Lam.*

§ 2. *Alsineæ.*

ARENARIA, *Linn.* (Sandwort.)
stricta, *Michx.*
serpyllifolia, *Linn.* Waukesha, *Mr. G. H. Cornwall !*
lateriflora, *Linn.*
STELLARIA, *Linn.* (Chickweed.)
media, *Smith.*
longifolia, *Muhl.* (Stichwort.)

CERASTIUM, *Linn.*
vulgatum, *Linn.* Beloit, *Mr. T. McEl Henry.*
viscosum, *Linn.* (Mouse-Ear.)

§ 3. *Illecebreæ.*

SPERGULA, *Linn.* (Spurrey.)
arvensis, *Linn.* Wawatosa, *Mr. M. Spears.*
ANYCHIA, *Michx.*
dichotoma, *Michx.* Blue Mounds, *I. A. Lapham.*

§ 4. *Mollugineæ.*

MOLLUGO, *Linn.*
verticillata, *Linn.* (Carpet weed.)

PORTULACACEÆ. (THE PURSLAIN FAMILY.)

PORTULACA, *Tourn.*
oleracea, *Linn.* (Purslain.)
TALINUM, *Adans.*
teretifolium, *Pursh.* Falls of the St. Croix, *Dr. Houghton.*
CLAYTONIA, *Linn.* (Spring Beauty.)
Virginica, *Linn.*

MALVACEÆ. (THE MALLOW FAMILY.)

MALVA, *Linn.* (Mallow.)
triangulata, *Leavenworth.* (*M. Houghtonii, Torr. & Gray.*)
Upper Mississippi, *Dr. Houghton.*

TILIACEÆ. (THE LINDEN FAMILY.)

TILIA, *Linn.*
Americana, *Linn.* (Basswood.)

LINACEÆ. (THE FLAX FAMILY.)

LINUM, *Linn.*
rigidum, *Pursh.*

GERANIACEÆ.

GERANIUM, *Linn.* (Cranesbill.)
maculatum, *Linn.*

(GERANIUM.)

Carolinianum, *Linn.*

Robertianum, *Linn.* Lake Superior to Upper Mississippi, *Dr. Houghton.*

OXALIDACEÆ. (THE WOOD SORREL FAMILY.)

OXALIS, *Linn.*

violacea, *Linn.* Rock Prairie, and near Beloit.

stricta, *Linn.*

BALSAMINACEÆ. (THE BALSAM FAMILY.)

IMPATIENS, *Linn.* (Jewel-Weed.)

pallida, *Nutt.*

fulva, *Nutt.*

LIMNANTHACEÆ.

FLŒRKEA, *Willd.* (False Mermaid.)

proserpinacoides, *Willd.*

ZANTHOXYLACEÆ. (THE PRICKLY ASH FAMILY.)

ZANTHOXYLUM, *Linn.*

Americanum, *Mill.* (Prickly Ash.)

PTELEA, *Linn.*

trifoliata, *Linn.*

ANACARDIACEÆ. (THE SUMACH FAMILY.)

RHUS, *Linn.* (Sumach.)

typhina, *Linn.*

glabra, *Linn.*

venenata, *DC.* (Poison Sumach.)

Toxicodendron, *Linn.* (Poison Oak.)

ACERACEÆ. (THE MAPLE FAMILY.)

ACER, *Linn.* (Maple.)

spicatum, *Lam.* (Mountain Maple.)

saccharinum, *Wang.* (Sugar Maple.)

rubrum, *Linn.* (Red Maple.)

NEGUNDO, *Mœnch.* (Box Maple.)
aceroides, *Mœnch.* Rock River and Sugar River, *I. A. L.*

CELASTRACEÆ. (THE SPINDLE-TREE FAMILY.)

STAPHYLEA, *Linn.* (Bladder-Nut.)
trifolia, *Linn.* Beloit. *Mr. T. Mc El Henry.*
CELASTRUS, *Linn.* (Bittersweet.)
scandens, *Linn.*
EUONYMUS, *Tourn.* (Spindle-Tree.)
atropurpureus, *Jacq.* (Here called Wahoo.)

RHAMNACEÆ. (THE BUCKTHORN FAMILY.)

RHAMNUS, *Linn.* (Buckthorn.)
alnifolius, *L'Her.*
CEANOTHUS, *Linn.* (New Jersey Tea.)
Americanus, *Linn.*
ovalis, *Bigel.* Beloit, *Mr. T. McEl Henry.*

VITACEÆ. (THE VINE FAMILY.)

VITIS, *Linn.* (Grape Vine.)
æstivalis, *Michx.* (Summer Grape.)
riparia, *Michx.* (Frost Grape.)
AMPELOPSIS, *Michx.* (Virginia Creeper.)
quinquefolia, *Michx.*

POLYGALACEÆ. (THE MILKWORT FAMILY.)

POLYGALA, *Tourn.* (Milkwort.)
incarnata, *Linn.* Beloit, *Mr. T. McEl Henry.*
sanguinea, *Linn.* (P. purpurea, *Nutt.*)
verticillata, *Linn.*
Senega, *Linn.* (Seneca Snake-Root.)
polygama, *Walt.* Waukesha, *Mr. G. H. Cornwall.*

LEGUMINOSÆ. (THE PEA FAMILY.)

VICIA, *Tourn.* (Vetch.)
Cracca, *Linn.* (Tufted Vetch.)
Americana, *Muhl.*

LATHYRUS, *Linn.*
- maritimus, *Bigel.*
- venosus, *Muhl.*
- ochroleucus, *Hook.*
- palustris, *Linn.*

APIOS, *Boerh.*
- tuberosa, *Mœnch.* (Indian Potato.)

AMPHICARPÆA, *Ell.*
- monoica, *Nutt.* (Wild Bean.)

DESMODIUM, *DC.* (Tick Trefoil.)
- acuminatum, *DC.*
- Canadense, *DC.*

LESPEDEZA, *Michx.* (Bush Clover.)
- violacea, Pers. var. *divergens*, Beloit, *Mr. T. McEl Henry.* Var. *sessiliflora* Waukesha, *I. A. Lapham.*
- capitata, *Michx.*

ASTRAGALUS, *Linn.* (Milk Vetch.)
- Canadensis, *Linn.*

PHACA, *Linn.* (Bladder Vetch.)
- neglecta, *Torr. & Gray.*

TEPHROSIA, *Pers.* (Hoary Pea.)
- Virginiana, *Pers.*

AMORPHA, *Linn.*
- fruticosa, *Linn.* Beloit, *Mr. T. McEl Henry.*
- canescens, *Nutt.* (Lead Plant.)

DALEA, *Linn.*
- laxiflora, *Pursh.* "Near Prairie du Chien," *Mr. Nuttall.*

PETALOSTEMON, *Michx.*
- violaceum, *Michx.*
- candidum, *Michx.*

TRIFOLIUM, *Linn.* (Clover.)
- pratense, *Linn.* (Red Clover.)
- repens, *Linn.* (White Clover.)

LUPINUS, *Tourn.* (Lupine.)
- perennis, *Linn.* (Wild Lupine.)

BAPTISIA, *Vent.* (False Indigo.)
- australis, *R. Brown.* (B. cærulea, *Nutt.*) On the Neenah River, *Dr. Houghton.*
- leucantha, *Torr. & Gray.* (B. alba, *Hooker.*)
- leucophæa, *Nutt.*

CASSIA, *Linn.* (Senna.)

Chamæcrista, *Linn.* Beloit, *Mr. T. McEl Henry.*

ROSACEÆ. (THE ROSE FAMILY.)

§ 1. *Amygdaleæ.*

PRUNUS, *Tourn.* (Plum.)

Americana, *Marsh.* (Yellow Plum.)

CERASUS, *Tourn.* (Cherry.)

pumila, *Michx.* (Sand Cherry.) Lakes Michigan and Superior, *Dr. Houghton.*

Pennsylvanica, *Loisel.* (Bird Cherry.)

Virginiana, *DC.* (Choke Cherry.)

serotina, *DC.* (Wild Black Cherry.)

§ 2. *Rosaceæ.*

SPIRÆA, *Linn.* (Meadowsweet.)

opulifolia, *Linn.* (Nine-Bark.)

salicifolia, *Linn.* (Meadowsweet.)

AGRIMONIA, *Tourn.* (Agrimony.)

Eupatoria, *Linn.*

GEUM, *Linn.* (Avens.)

Virginianum, *Linn.* (White Avens.)

macrophyllum, *Willd.* Lake Superior, *Dr. Z. Pitcher.*

strictum, *Ait.*

rivale, *Linn.* (Water, or Purple Avens.)

triflorum, *Pursh.*

POTENTILLA, *Linn.*

Norvegica, *Linn.*

Canadensis, *Linn.* (Fivefinger.)

arguta, *Pursh.*

anserina, *Linn.* (Silver-Leaf.)

fruticosa, *Linn.*

tridentata, *Ait.* Lake Superior, *Dr. Houghton.*

COMARUM, *Linn.* (Marsh Fivefinger.)

palustre, *Linn.*

FRAGARIA, *Tourn.* (Strawberry.)

Virginiana, *Ehrh.*

vesca, *Linn.*

RUBUS, *Linn.* (Bramble.)

Nutkanus, *Mocino.* Head of Lake Superior, *Dr. Houghton.*

(RUBUS.)
triflorus, *Richards.*
strigosus, *Michx.* (Red Raspberry.)
occidentalis, *Linn.* (Black Raspberry.)
villosus, *Ait.* (Blackberry.)
hispidus, *Linn.* Lake Superior, *Dr. Houghton.*
ROSA, *Tourn.* (Rose.)
lucida, *Ehrh.* (Wild Rose.)
blanda, *Ait.* Lake Superior, *Dr. Houghton.*

§ 3. *Pomeæ.*

CRATÆGUS, *Linn.*
coccinea, *Linn.* (White Thorn.)
punctata, *Jacq.*
PYRUS, *Linn.* (Apple.)
coronaria, *Linn.* (Crab-Apple.)
arbutifolia, *Linn.* (Choke Berry.)
Americana, *DC.* (Mountain Ash.) Brookfield, Waukesha Co. *Mr. G. H. Cornwall.*
AMELANCHIER, *Medic.* (June-Berry.)
Canadensis, *Torr. & Gray.*

LYTHRACEÆ. (THE LOOSESTRIFE FAMILY.)

LYTHRUM, *Linn.* (Loosestrife.)
alatum, *Pursh.*

ONAGRACEÆ. (THE EVENING PRIMROSE FAMILY.)

§ 1. *Onagraceæ.*

EPILOBIUM, *Linn.*
angustifolium, *Linn.* (Willow Herb.)
coloratum, *Muhl.*
palustre, *Linn.*
ŒNOTHERA, *Linn.* (Evening Primrose.)
biennis, *Linn.*
chrysantha, *Michx.* Waukesha, *Mr. G. H. Cornwall.*
LUDWIGIA, *Linn.*
palustris, *Ell.* (Water Purslain.)
CIRCÆA, *Tourn.* (Enchanter's Nightshade.)
Lutetiana, *Linn.*
alpina, *Linn.*

§ 2. *Halorageæ.*

MYRIOPHYLLUM, *Vaill.* (Water Milfoil.)
spicatum, *Linn.*
verticillatum, *Linn.*

HIPPURIS, *Linn.* (Mares-Tail.)
vulgaris, *Linn.*

GROSSULACEÆ. (THE GOOSEBERRY FAMILY.)

RIBES, *Linn.*
Cynosbati, *Linn.* (Prickly Gooseberry.)
hirtellum, *Michx.* (Smooth Gooseberry.)
rotundifolium, *Michx.* (Swamp Gooseberry.)
floridum, *Linn.* (Wild Black Currant.)
rubrum, *Linn.* (Wild Red Currant.)

CUCURBITACEÆ. (THE CUCUMBER FAMILY.)

ECHINOCYSTIS, *Torr. & Gray.*
lobata, *Torr. & Gray.* (Wild Cucumber.)

CRASSULACEÆ. (THE HOUSE-LEEK FAMILY.)

PENTHORUM, *Gronov.*
sedoides, *Linn.* (Stonecrop.)

SAXIFRAGACEÆ. (THE SAXIFRAGE FAMILY.)

SAXIFRAGA, *Linn.* (Saxifrage.)
Aizoon, *Jacq.* Lake Superior, *Dr. Z. Pitcher.*
Virginiensis, *Michx.* Lake Superior, *Dr. Houghton.*
Pennsylvanica, *Linn.*

HEUCHERA, *Linn.* (Alum Root.)
Americana, *Linn.*

MITELLA, *Tourn.* (Mitrewort.)
diphylla, *Linn.* (Currant Leaf.)
nuda, *Linn.*

CHRYSOSPLENIUM, *Tour.* (Golden Saxifrage.)
Americanum, *Schw.* Lake Superior to Upper Mississippi, *Dr. Houghton.*

HAMAMELACEÆ. (THE WITCH HAZEL FAMILY.)

HAMAMELIS, *Linn.* (Witch Hazel.)
Virginica, *Linn.*

UMBELLIFERÆ.

SANICULA, *Tourn.*
Marylandica, *Linn.* (Sanicle.)
ERYNGIUM, *Tourn.*
aquaticum, *Linn.* (Rattlesnake-Master.)
POLYTÆNIA, *DC.*
Nuttallii, *DC.*
HERACLEUM, *Linn.* (Cow Parsnip.)
lanatum, *Michx.*
PASTINACA, *Tourn.*
sativa, *Linn.* (Wild Parsnip. Poisonous.)
ARCHANGELICA, *Hoffm.*
atropurpurea, *Hoffm.*
CONIOSELINUM, *Fischer.*
Canadense, *Torr. & Gray.*
ZIZIA, *Koch.* (Alexanders.)
cordata, *Koch.*
aurea, *Koch.*
integerrima, *DC.*
BUPLEURUM, *Tourn.*
rotundifolium, *Linn.* (Introduced.)
CICUTA, *Linn.*
maculata, *Linn.*
bulbifera, *Linn.*
SIUM, *Linn.* (Water Parsnip.)
latifolium, *Linn.*
CRYPTOTÆNIA, *DC.*
Canadense, *DC.* (Honewort.)
OSMORRHIZA, *Raf.* (Sweet Cicely.)
longistylis, *DC.*
CONIUM, *Linn.* (Poison Hemlock.)
maculatum, *Linn.* Green Bay.
ERIGENIA, *Nutt.*
bulbosa, *Nutt.*

ARALIACEÆ. (THE SPIKENARD FAMILY.)

ARALIA, *Linn.*
racemosa, *Linn.* (Spikenard.)
nudicaulis, *Linn.* (Wild Sarsaparilla.)

PANAX, *Linn.*
quinquefolium, *Linn.* (Ginseng.)
trifolium, *Linn.* (Ground-Nut.)

CORNACEÆ. (THE DOGWOOD FAMILY.)

CORNUS, *Tourn.*
alternifolia, *Linn.* (Yellow-Twigged Dogwood.)
circinata, *L'Her.* L. Superior to Upper Miss. *Dr. Houghton.*
sericea, *Linn.*
stolonifera, *Michx.* (Red-Twigged Dogwood.)
paniculata, *L'Her.*
Canadensis, *Linn.* (Pudding Berry.)

CAPRIFOLIACEÆ. (THE HONEYSUCKLE FAMILY.)

LINNÆA, *Gronov.* (Twin-Flower.)
borealis, *Gronov.*
SYMPHORICARPUS, *Dill.*
occidentalis, *R. Brown.* (Wolf-Berry.)
racemosus, *Michx.* (Snowberry.)
LONICERA, *Linn.* (Honeysuckle.)
sempervirens, *Ait.* Lake Superior, *Dr. Houghton.*
flava, *Sims.* (Yellow Honeysuckle.)
parviflora, *Lam.*
hirsuta, *Eaton.* L. Superior to Upper Miss. *Dr. Houghton.*
ciliata, *Muhl.*
cærulea, *Linn.*
DIERVILLA, *Tourn.*
trifida, *Mœnch.*
TRIOSTEUM, *Linn.* (Horse-Gentian.)
perfoliatum, *Linn.*
SAMBUCUS, *Linn.* (Elder.)
Canadensis, *Linn.*
VIBURNUM, *Linn.*
Lentago, *Linn.*
dentatum, *Linn.* (Arrowwood.)
acerifolium, *Linn.*
Opulus, *Linn.* V. oxycoccus, *Pursh.* (High Cranberry.)

RUBIACEÆ. (The Madder Family.)

§ 1. *Stellatæ.*

Galium, *Linn.*
 Aparine, *Linn.* (Goose-Grass.)
 asprellum, *Michx.*
 trifidum, *Linn.*
 triflorum, *Michx.*
 lanceolatum, *Torr.* (Wild Liquorice.)
 boreale, *Linn.*

§ 2. *Cinchoneæ.*

Cephalanthus, *Linn.* (Button Bush.)
 occidentalis, *Linn.*
Mitchella, *Linn.* (Partridge-Berry.)
 repens, *Linn.*
Hedyotis, *Linn.*
 ciliolata, *Torr.*
 longifolia, *Hook.* St. Louis River, *Dr. Houghton.*

VALERIANACEÆ. (The Valerian Family.)

Valeriana, *Tourn.* (Valerian.)
 ciliata, *Torr. & Gray.*
Fedia, *Gærtn.*
 Fagopyrum, *Torr. & Gray.*

COMPOSITEÆ.

Vernonia, *Schreb.*
 Noveboracensis, *Willd.* Upper Mississippi, *Dr. Houghton.*
 fasciculata, *Michx.* Beloit, *Mr. T. McEl Henry.*
Liatris, *Schreb.*
 cylindracea, *Michx.*
 scariosa, *Willd.*
 spicata, *Willd.*
Kuhnia, *Linn.*
 eupatorioides, *Linn.*
Eupatorium, *Tourn.*
 purpureum, *Linn.*
 perfoliatum, *Linn.* (Thoroughwort.)
 ageratoides, *Linn.*

ADENOCAULON, *Hook.*
bicolor, *Hook.* Lake Superior, *Dr. Z. Pitcher.*
ASTER, *Linn.* (Starwort.)
macrophyllus, *Linn.*
sericeus, *Vent.*
concolor, *Linn.* Neenah River, *Dr. Houghton.*
lævis, *Linn.*
azureus, *Lindl.*
Shortii, *Boott.*
sagittifolius, *Willd.*
multiflorus, *Ait.*
miser, *Linn.*
ASTER TENUIFOLIUS, *Linn.* Upper Mississippi, *Dr. Houghton.*
præaltus, *Poir.*
laxifolius, *Nees.*
puniceus, *Linn.*
prenanthoides, *Muhl.*
oblongifolius, *Nutt.* Upper Mississippi, *Torr. & Gray.*
Novæ-Angliæ, *Linn.*
ptarmicoides, *Torr. & Gray.*
ERIGERON, *Linn.* (Fleabane.)
Canadense, *Linn.*
bellidifolium, *Muhl.* (Roberts's Plantain.)
Philadelphicum, *Linn.*
glabellum, *Nutt.* St. Croix River, *Dr. Houghton.*
strigosum, *Muhl.*
DIPLOPAPPUS, *Cass.*
linariifolius, *Hook.* Beloit, *Mr. T. McEl Henry.*
umbellatus, *Torr. & Gray.*
SOLIDAGO, *Linn.* (Golden Rod.)
latifolia, *Linn.*
speciosa, *Nutt.*
Virga-aurea, *Linn.* Lake Superior, *Dr. Houghton.*
rigida, *Linn.*
Ohioensis, *Riddell.*
Riddellii, *Frank.*
neglecta, *Torr. & Gray.*
patula, *Muhl.*
arguta, *Ait.*
altissima, *Linn.*

(SOLIDAGO)

ulmifolia, *Muhl.*

nemoralis, *Ait.*

Canadensis, *Linn.*

lanceolata, *Linn.*

CHRYSOPSIS, *Nutt.*

villosa, *Nutt.* Upper Mississippi, *Dr. Houghton.*

POLYMNIA, *Linn.*

Canadensis, *Linn.*

SILPHIUM, *Linn.*

ternatum, *Linn.* Waukesha, *Mr. G. H. Cornwall.*

laciniatum, *Linn.* (Compass Plant.*)

terebinthinaceum, *Linn.* (Prairie Dock.)

trifoliatum, *Linn.* Waukesha, *Mr. G. H. Cornwall.*

integrifolium, *Michx.*

perfoliatum, *Linn.* Waukesha, *Mr. G. H. Cornwall.*

PARTHENIUM, *Linn.*

integrifolium, *Linn.*

AMBROSIA, *Tourn.*

trifida, *Linn.*

artemisiæfolia, *Linn.*

XANTHIUM, *Tourn.*

strumarium, *Linn.*

HELIOPSIS, *Pers.* (Oxeye.)

lævis, *Pers.*

ECHINACEA, *Mœnch.*

angustifolia, *DC.* Beloit, *Mr. T. McEl Henry.*

RUDBECKIA, *Linn.*

laciniata, *Linn.*

hirta, *Linn.*

LEPACHYS, *Raf.*

pinnata, *Torr. & Gray.*

HELIANTHUS, *Linn.* (Sunflower.)

rigidus, *Pers.*

occidentalis, *Riddell.*

giganteus, *Linn.*

decapetalus, *Linn.*

strumosus, *Linn.*

* The note upon the direction which the leaves of this plant assume, on the prairies, has already been printed, on page 16.

CORREOPSIS, *Linn.*
trichosperma, *Michx.*
palmata, *Nutt.*
BIDENS, *Linn.* (Beggar-Ticks.)
frondosa, *Linn.*
cernua, *Linn.*
chrysanthemoides, *Michx.*
Beckii, *Torr.* St. Croix River, *Dr. Houghton.*
HELENIUM, *Linn.*
autumnale, *Linn.*
MARUTA, *Cass.* (May-Weed.)
Cotula, *DC.*
ACHILLEA, *Linn.* (Yarrow.)
Millefolium, *Linn.*
TANACETUM, *Linn.* (Tansey.)
Huronensis, *Nutt.* Lake Superior, *Dr. Houghton.*
ARTEMISIA, *Linn.* (Wormwood.)
Canadensis, *Michx.*
Ludoviciana, *Nutt.*
biennis, *Willd.*
GNAPHALIUM, *Linn.*
polycephalum, *Michx.*
ANTENNARIA, *Gærtn.*
dioica, *Gærtn.*
plantaginifolia, *Hook.*
CACALIA, *Linn.* (Indian Plantain.)
suaveolens, *Linn.*
reniformis, *Muhl.*
atriplicifolia, *Linn.*
tuberosa, *Nutt.*
SENECIO, *Linn.*
aureus, *Linn.* (Ragwort.)
tomentosus, *Michx.* Beloit, *Mr. T. McEl Henny.*
CIRSIUM, *Tourn.*
lanceolatum, *Scop.* (Thistle, introduced.)
Pitcheri, *Torr. & Gray.* Lake Superior, *Dr. Z. Pitcher.*
Virginianum, *Michx.*
muticum, *Michx.*
pumilum, *Spreng.*
arvense, *Scop.* (Canada Thistle, introduced.)

LAPPA, *Tourn.*
major, *Gærtn.* (Burdock.)
CYNTHIA, *Don.*
Virginica, *Don.*
HIERACIUM, *Tourn.* (Hawkweed.)
Canadense, *Michx.*
scabrum, *Michx.*
Gronovii, *Linn.*
NABALUS, *Cass.*
albus, *Hook.*
racemosus, *Hook.*
TROXIMON, *Nutt.*
cuspidatum, *Pursh.*
TARAXACUM, *Haller.*
Dens-leonis, *Desf.* (Dandelion.)
LACTUCA, *Tourn.*
elongata, *Muhl.*
SONCHUS, *Linn.* (Sow-Thistle.)
oleraceus, *Linn.*

LOBELIACEÆ. (THE LOBELIA FAMILY.)

LOBELIA, *Linn.*
cardinalis, *Linn.* (Cardinal Flower.)
siphilitica, *Linn.* (Blue Lobelia.)
inflata, *Linn.* (Indian Tobacco.)
spicata, *Lam.*
Kalmii, *Linn.*

CAMPANULACEÆ. (THE BELL-FLOWER FAMILY.)

CAMPANULA, *Tourn.*
rotundifolia, *Linn.* (Hair-Bell.)
aparinoides, *Pursh.*
Americana, *Linn.*

ERICACEÆ. (THE HEATH FAMILY.)

§ 1. *Vacciniea.*

GAYLUSSACIA, *H. B. & K.* (Huckleberry.)
resinosa, *Torr. & Gray.* (Black Huckleberry.)
VACCINIUM, *Linn.*
macrocarpon, *Ait.* (Cranberry.)
Pennsylvanicum, *Lam.* (Blue Huckleberry.)

CHIOGENES, *Salisb.*
hispidula, *Torr. & Gray.*

§ 2. *Ericineæ.*

ARCTOSTAPHYLOS, *Adans.*
Uva-ursi, *Spreng.* (Bear-Berry.)
GAULTHERIA, *Kalm.*
procumbens, *Linn.* (Winter-Green.)
ANDROMEDA, *Linn.*
polifolia, *Linn.*
calyculata, *Linn.*
KALMIA, *Linn.* (Laurel.)
glauca, *Ait.* Lake Superior, *Prof. Douglass.*
LEDUM, *Linn.* (Labrador Tea.)
latifolium, *Ait.* L. Superior to Upper Miss., *Dr. Houghton.*

§ 3. *Pyroleæ.*

PYROLA, *Linn.*
rotundifolia, *Linn.*
asarifolia, *Michx.*
elliptica, *Nutt.* (Shin-Leaf.)
secunda, *Linn.*
MONESES, *Sailsb.* (Pyrola, *Linn.*)
uniflora. Mauvaise River, *Dr. Houghton.*
CHIMAPHILA, *Pursh.*
umbellata, *Nutt.* (Prince's Pine.)

§ 4. *Monotropeæ.*

MONOTROPA, *Gronov.* (Indian Pipe.)
uniflora, *Linn.*

AQUIFOLIACEÆ. (THE HOLLY FAMILY.)

PRINOS, *Linn.* (Winter-Berry.)
verticillatus, *Linn.*
NEMOPANTHES, *Raf.*
Canadensis, *DC.* Lake Superior, *Dr. Houghton.*

PLANTAGINACEÆ. (THE PLANTAIN FAMILY.)

PRANTAGO, *Linn.* (Plantain.)
major, *Linn.*
cordata, *Lam.*

PRIMULACEÆ. (The Primrose Family.)

Primula, *Linn.* (Primrose.)
 farinosa, *Linn.* Lake Superior, *Dr. Houghton.*
 Mistassinica, *Michx.* Lake Superior, *Dr. Houghton.*
Dodecatheon, *Linn.*
 Meadia, *Linn.* (Shooting Star.)
Trientalis, *Linn.*
 Americana, *Pursh.*
Lysimachia, *Linn.* (Loosestrife.)
 quadrifolia, *Linn.*
 ciliata, *Linn.*
 lanceolata, *Walt.* (L. hybrida, *Michx.*) Brookfield, *Mr. M. Spears.*
 angustifolia, *Lam.* (L. revoluta, *Nutt.*)
Naumburgia, *Mœnch.*
 thyrsiflora, *Reich.*

LENTIBULACEÆ. (The Bladderwort Family.)

Utricularia, *Linn.*
 purpurea, *Walt.* Lac Chetac, *Dr. Houghton.*
 vulgaris, *Linn.*
 minor, *Linn.*
 intermedia, *Hayne.* Brookfield, *Mr. M. Spears.*
 cornuta, *Michx.* Lake Superior, *Dr. Houghton.*

OROBANCHACEÆ. (The Broom Rape Family.)

Epiphegus, *Nutt.* (Beech Drops.)
 Virginiana, *Bart.*
Conopholis, *Wallr.* (Orobanche, *Linn.*)
 Americana, *Wallr.*

ACANTHACEÆ. (The Acanthus Family.)

Dipteracanthus, *Nees.* (Ruellia, *Linn.*)
 strepens, *Nees.* Beloit, *Mr. T. McEl Henny.*

SCROPHULARIACEÆ. (The Figwort Family.)

Verbascum, *Linn.* (Mullen.)
 Thapsus, *Linn.* (Introduced.)

LINARIA, *Tourn.* (Toad Flax.)
vulgaris, *Mill.* Waukesha, *Mr. G. H. Cornwall.*
SCROPHULARIA, *Linn.* (Figwort.)
nodosa, *Linn.* (S. Marylandica.)
COLLINSIA, *Nutt.*
verna, *Nutt.*
parviflora, *Dougl.* South shore of L. Superior, *Dr. Z. Pitcher.*
CHELONE, *Tourn.* (Snake Head.)
glabra, *Linn.*
PENTSTEMON, *Mitchell.*
pubescens, *Solander.*
MIMULUS, *Linn.* (Monkey Flower.)
ringens, *Linn.*
Jamesii, *Torr. & Gray.*
SYNTHYRIS, *Benth.*
Houghtoniana, *Benth.*
VERONICA, *Linn.* (Speedwell.)
Virginica, *Linn.* (Leptandra, *Nutt.*)
Anagalis, *Linn.*
scutellata, *Linn.*
peregrina, *Linn.*
GERARDIA, *Linn.*
purpurea, *Linn.*
tenuifolia, *Vahl.*
setacea, *Waltr.*
pedicularia, *Linn.* Wisconsin River, *Prof. Douglass.* Neenah River, *Dr. Houghton.*
grandiflora, *Benth.*
CASTILLEJA, *Mutis.* (Painted Cup.)
coccinea, *Spreng.*
sessiliflora, *Pursh.* (Euchroma grandiflora, *Nutt.*)
PEDICULARIS, *Tourn.* (Lousewort.)
Canadensis, *Linn.*
lanceolata, *Michx.*
MELAMPYRUM, *Tourn.* (Cow Wheat.)
pratense, *Linn.* (M. Americanum, *Michx.*)

VERBENACEÆ. (THE VERVAIN FAMILY.)

VERBENA, *Linn.* (Vervain.)
hastata, *Linn.*

(Verbena)
urticifolia, *Linn.*
angustifolia, *Linn.*
bracteosa, *Michx.*
stricta, *Vent.* Upper Mississippi, *Dr. Houghton.*
Phryma, *Linn.* (Lopseed.)
leptostachya, *Linn.*

LABIATÆ. (The Mint Family.)

Mentha, *Linn.* (Mint.)
Canadensis, *Linn.* (M. borealis, *Michx.*)
Lycopus, *Linn.*
Virginicus, *Linn.* (Bugle Weed.)
sinuatus, *Ell.* (L. Europæus, *Ph.*)
Micromeria, *Benth.*
glabella, *Benth.* (Hedeoma glabra, *Nutt.*) Lake Michigan to Lake Superior and Upper Mississippi, *Dr. Houghton.*
Blephilla, *Raf.*
ciliata, *Raf.*
Monarda, *Linn.* (Horse Mint.)
fistulosa, *Linn.*
punctata, *Linn.*
Nepeta, *Linn.* (Catnip.)
cataria, *Linn.*
Lopanthus, *Benth.*
nepetoides, *Benth.*
scrophulariæfolius, *Benth.*
Pycnanthemum, *Michx.* (Mountain Mint.)
lanceolatum, *Pursh.*
Prunella, *Linn.* (Heal-All.)
vulgaris, *Linn.*
Scutellaria, *Linn.* (Scullcap.)
parvula, *Michx.*
galericulata, *Linn.*
lateriflora, *Linn.* (Mad Dog Scullcap.)
cordifolia, *Muhl.*
Physostegia, *Benth.* (Dracocephalum, *Linn.*)
Virginiana, *Benth.* (Dragon-Head.)
Leonurus, *Linn.* (Motherwort.)
Cardiaca, *Linn.*

STACHYS, *Linn.* (Horse Nettle.)
aspera, *Michx.*
TEUCRIUM, *Linn.* (Germander.)
Canadense, *Linn.*

BORAGINACEÆ. (THE BORAGE FAMILY.)

ONOSMODIUM, *Michx.*
Virginianum, *DC.* (O. hispidum, *Mx.*)
LITHOSPERMUM, *Tourn.* (Gromwell.)
officinale, *Linn.*
canescens, *Lehm.* (Batschia canescens, *Mx.*)
PENTALOPHUS, *DC.* (Batschia, *Nutt.*)
longiflorus, *DC.* Beloit, *Mr. T. McEl Henny.*
MERTENSIA, *Roth.* Pulmonaria, *Linn.* (Lungwort.)
Virginica, *DC.* Beloit, *Mr. T. McEl Henny.*
ECHINOSPERMUM, *Swartz.*
Lappula, *Lehm.*
CYNOGLOSSUM, *Tourn.* (Hound's-Tongue.)
officinalis, *Linn.*
Virginicum, *Linn.*
Morisoni, *DC.*

HYDROPHYLLACEÆ. (THE WATER-LEAF FAMILY.)

HYDROPHYLLUM, *Linn.* (Water-Leaf.)
Virginicum, *Linn.*
appendiculatum, *Michx.* Beloit, *Mr. T. McEl Henny.*

POLEMONIACEÆ.

POLEMONIUM, *Tourn.*
reptans, *Linn.* (Jacob's Ladder.)
PHLOX, *Linn.*
glaberrima, *Linn.* (P. revoluta, *Eaton.*)
pilosa, *Linn.*
divaricata, *Linn.*

CONVOLVULACEÆ.

CALYSTEGIA, *R. Brown.* (Convolvulus, *Linn.*)
sepium, *R. Brown.*
spithamæa, *Pursh.*

CUSCUTA, *Tourn.*
Gronovii, *Willd.* (C. Americana, *Pursh.*)

SOLANACEÆ. (THE NIGHTSHADE FAMILY.)

DATURA, *Linn.* (Jamestown Weed.)
Stramonium, *Linn.* (Introduced.)
NICANDRA, *Adans.*
physaloides, *Gærtn.*
PHYSALIS, *Linn.* (Ground Cherry.)
viscosa, *Linn.*
SOLANUM, *Linn.*
nigrum, *Linn.* (Night Shade.)

GENTIANACEÆ. (THE GENTIAN FAMILY.)

GENTIANA, *Linn.*
quinqueflora, *Lam.*
crinita, *Fræl.*
detonsa, *Fries.*
rubricaulis, *Keating.* Waukesha, *Mr. G. H. Cornwall.*
saponaria, *Linn.*
HALENIA, *Borkh.* (Swertia, *Linn.*)
deflexa, *Griesb.* Mauvaise and Bois Brulé Rivers, *Dr. Houghton.*
MENYANTHES, *Tourn.* (Buckbean.)
trifoliata, *Linn.*

APOCYNACEÆ. (THE DOGBANE FAMILY.)

APOCYNUM, *Tourn.*
androsæmifolium, *Linn.*
hypericifolium, *Ait.*

ASCLEPIADACEÆ. (THE MILKWEED FAMILY.)

ASCLEPIAS, *Linn.*
Cornuti, *Decaisne.* (A. Syriaca, *Linn.*)
phytolaccoides, *Pursh.*
variegata, *Linn.*
obtusifolia, *Michx.*
rubra, *Linn.* Waukesha, *Mr. G. H. Cornwall.*
incarnata, *Linn.*

(ASCLEPIAS)
tuberosa, *Linn.* (Butterfly Weed.)
lanuginosa, *Nutt.* Eagle Prairie, *I. A. Lapham.*
ACERATES, *Ell.*
viridiflora, *Ell.*

OLEACEÆ. (THE OLIVE FAMILY.)

FRAXINUS, *Tourn.* (Ash.)
Americana, *Linn.* (White Ash.)
sambucifolia, *Lam.* (Black Ash.)

ARISTOLOCHIACEÆ. (THE BIRTHWORT FAMILY.)

ASARUM, *Tourn.*
Canadense, *Linn.* (Colt's Foot.)

CHENOPODIACEÆ. (THE GOOSEFOOT FAMILY.)

SALSOLA, *Linn.* (Saltwort.)
kali, *Linn.* Lake Shore, Milwaukie.
CHENOPODIUM, *Linn.*
album, *Linn.* (Pigweed.)
hybridum, *Linn.* (Goosefoot.)
BLITUM, *Tourn.*
capitatum, *Linn.* (Indian Strawberry.)
AMBRINA, *Spach.*
Botrys, *Moquin.* Brookfield, *Mr. G. H. Cornwall.*

AMARANTHACEÆ. (THE AMARANTH FAMILY.)

AMARANTHUS, *Linn.*
altissimus, *Riddell.*
hybridus, *Linn.* (Red Root.)
hypocondriacus, *Linn.* (Prince's Feather.)
tamariscinus, *Nutt.*

NYCTAGINACEÆ. (THE NYCTAGO FAMILY.)

ALLIONIA, *Linn.*
albida, *Wallr.* St. Croix River, *Dr. Houghton.*
nyctaginea, *Michx.* St. Croix River, *Dr. Houghton.*

POLYGONACEÆ. (The Buckwheat Family.)

Polygonum, *Linn.*
- Pennsylvanicum, *Linn.*
- Persicaria, *Linn.*
- Hydropiper, *Linn.* (P. punctatum, *Ell.*)
- hydropiperoides, *Michx.*
- amphibium, *Linn.*
- aviculare, *Linn.* (Knot-Grass.)
- articulatum, *Linn.* Lakes Michigan and Superior, *Dr. Houghton.*
- Virginianum, *Linn.*
- sagittatum, *Linn.*
- Convolvulus, *Linn.*
- cilinode, *Michx.* Lake Superior, *Dr. Houghton.*

Rumex, *Linn.* (Dock.)
- Britannica, *Linn.*
- crispus, *Linn.* (Curled Dock.)
- Acetosella, *Linn.* (Sorrel.)

THYMELEACEÆ. (The Mezereum Family.)

Dirca, *Linn.* (Leatherwood.)
- palustris, *Linn.*

ELÆAGNACEÆ. (The Oleaster Family.)

Shepherdia, *Nutt.*
- Canadensis, *Nutt.*

SANTALACEÆ. (The Sandal Wood Family.)

Comandra, *Nutt.*
- umbellata, *Nutt.*

ULMACEÆ. (The Elm Family.)

Ulmus, *Linn.* (Elm.)
- Americana, *Linn.* (White Elm.)
- fulva, *Michx.* (Slippery Elm.)

Celtis, *Tourn.*
- occidentalis, *Linn.* (Hack Berry.)

SAURURACEÆ. (The Lizard's Tail Family.)

Saururus, *Linn.* (Lizard's Tail.)
cernuus, *Linn.* Upper Mississippi, *Dr. Houghton.*

CALLITRICHACEÆ.

Callitriche, *Linn.*
verna, *Linn.*

EUPHORBIACEÆ. (The Spurge Family.)

Euphorbia, *Linn.*
corollata, *Linn.*
maculata, *Linn.*

EMPETRACEÆ. (The Crowberry Family.)

Empetrum, *Tourn.* (Crowberry.)
nigrum, *Linn.* Lake Superior, *Dr. Houghton.*

JUGLANDACEÆ. (The Walnut Family.)

Juglans, *Linn.* (Walnut.)
cinerea, *Linn.* (Butternut or White-Walnut.)
nigra, *Linn.* (Black-Walnut.)
Carya, *Nutt.* (Hickory.)
alba, *Nutt.* (Shag-Bark Hickory.)
glabra, *Torr.* (C. porcina, *Nutt.*) (Pignut Hickory.)

CUPULIFERÆ. (The Oak Family.)

Quercus, *Linn.* (Oak.)
alba, *Linn.* (White Oak.)
obtusiloba, *Michx.* (Q. stellata, *Willd.*) (Post Oak.) Upper Mississippi, *Dr. Houghton.*
macrocarpa, *Michx.* (Burr Oak.)
bicolor, *Willd.* (Swamp White Oak.)
Prinos, *Linn.* (Swamp Chestnut Oak.) Near Janesville, *I. A. L.*
rubra, *Linn.* (Red Oak.)
palustris, *Linn.* (Pin Oak.)
Fagus, *Tourn.* (Beech.)
ferruginea, *Ait.*

CORYLUS, *Tourn.* (Hazel-Nut.)
 Americana, *Walt.*
 rostrata, *Ait.* Lake Superior to Upper Miss., *Dr. Houghton.*
CARPINUS, *Linn.* (Hornbeam.)
 Americana, *Michx.*
OSTRYA, *Micheli.* (Iron Wood.)
 Virginica, *Willd.*

MYRICACEÆ. (THE SWEET GALE FAMILY.)

MYRICA, *Linn.* (Sweet Gale.)
 Gale, *Linn.* Lake Superior, *Dr. Houghton.*

BETULACEÆ. (THE BIRCH FAMILY.)

BETULA, *Tourn.* (Birch.)
 papyracea, *Ait.* (Canoe Birch.)
 pumila, *Linn.* (B. glandulosa, *Mx.*) (Low Birch.)
ALNUS, *Tourn.* (Alder.)
 incana, *Willd.* (A. glauca, *Mx.*) St. Croix River, *Dr. Houghton.*
 serrulata, *Willd.*

SALICACEÆ. (THE WILLOW FAMILY.)

SALIX, *Tourn.* (Willow.)
 candida, *Willd.*
 tristis, *Ait.*
 humilis, *Marshall.* (S. Muhlenbergiana, *Barr.*)
 discolor, *Muhl.*
 eriocephala, *Michx.* (S. prinoides, *Ph.*) Mauvaise River, *Dr. Houghton.*
 sericea, *Marshall.* (S. grisea, *Willd.*)
 rostrata, *Richardson.*
 lucida, *Muhl.*
 longifolia, *Muhl.* Upper Mississippi, *Dr. Houghton.*
POPULUS, *Tourn.* (Poplar.)
 tremuloides, *Michx.* (Quaking Aspen.)
 grandidentata, *Michx.*
 candicans, *Ait.* (Balm of Gilead.)

PLATANACEÆ. (THE PLANE-TREE FAMILY.)

PLATANUS, *Linn.* (Sycamore, Buttonwood.)
 occidentalis, *Linn.*

URTICACEÆ. (The Nettle Family.)

Humulus, *Linn.* (Hop.)
 Lupulus, *Linn.*

Urtica, *Tourn.* (Nettle.)
 dioica, *Linn.*
 Canadensis, *Linn.*

Pilea, *Lindl.* (Adike, *Raf.*)
 pumila, *Linn.*

Parietaria, *Tourn.* (Pellitory.)
 Pennsylvanica, *Muhl.*

CONIFERÆ. (The Pine Family.)

Pinus, *Tourn.* (Pine.)
 Banksiana, *Lambert.* Upper Mississippi, *Dr. Houghton.*
 resinosa, *Ait.* (Red Pine.)
 mitis, *Michx.* (Yellow Pine.) Dane County, *I. A. L.*
 Strobus, *Linn.* (White Pine.)

Abies, *Tourn.* (Spruce.)
 balsamea, *Marsh.* (Balsam Fir.) Manitowoc, *I. A. L.*
 Canadensis, *Michx.* (Hemlock.) Manitowoc, *I. A. L.*
 nigra, *Poir.* (Black Spruce.) Lake Superior, *Dr. Houghton.* Upper Mississippi, *Prof. Douglass.*

Larix, *Tourn.* (Larch.)
 Americana, *Michx.* (Tamarack.)

Cupressus, *Tourn.*
 thyoides, *Linn.* (White Cedar.)

Juniperus, *Linn.* (Juniper.)
 communis, *Linn.*
 Virginiana, *Linn.* (Red Cedar.)

Taxus, *Tourn.* (Yew.)
 Canadensis, *Willd.*

ARACEÆ. (The Arum Family.)

Arum, *Linn.* (Indian Turnip.)
 triphyllum, *Linn.*

Calla, *Linn.* (Water Arum.)
 palustris, *Linn.*

Symplocarpus, *Salisb.* (Skunk Cabbage.)
 fœtidus, *Salisb.*

ACORUS, *Linn.* (Sweet Flag.)
calamus, *Linn.*

LEMNACEÆ. (THE DUCKWEED FAMILY.)

LEMNA, *Linn.* (Duckweed.)
minor, *Linn.*
trisulca, *Linn.*

TYPHACEÆ. (THE CAT-TAIL FAMILY.)

TYPHA, *Tourn.* (Cat-Tail.)
latifolia, *Linn.*
SPARGANIUM, *Tourn.*
ramosum, *Hudson.*

NAIDACEÆ. (THE PONDWEED FAMILY.)

POTAMOGETON, *Tourn.* (Pondweed.)
amplifolius, *Tuckerman.*
perfoliatus, *Linn.*
compressus, *Linn.* (P. zosterifolius, *Schum.*)
pauciflorus, *Pursh.* (P. gramineus, *Mx.*)
pectinatus, *Linn.*

ALISMACEÆ. (THE WATER PLANTAIN FAMILY.)

§ 1. *Juncagineæ.*

TRIGLOCHIN, *Linn.*
elatum, *Nutt.*
SCHEUCHZERIA, *Linn.*
palustris, *Linn.*

§ 2. *Alismeæ.*

ALISMA, *Linn.* (Water Plantain.)
Plantago, *Linn.*
SAGITTARIA, *Linn.*
sagittifolia, *Linn.* (Arrow Leaf.)

HYDROCARDIACEÆ. (THE FROG'S-BIT FAMILY.)

UDORA, *Nutt.* (Water Weed.)
Canadensis, *Nutt.*
VALLISNERIA, *Mitcheli.* (Tape Grass.)
spiralis, *Linn.*

ORCHIDACEÆ. (The Orchis Family.)

Microstylis, *Nutt.*
ophioglossoides, *Nutt.*
Liparis, *Richards.*
liliifolia, *Richards.*
Lœselii, *Richards.* Waukesha, *Mr. G. H. Cornwall.*
Corallorhiza, *Haller.*
multiflora, *Nutt.* Waukesha, *Mr. G. H. Cornwall.*
Aplectrum, *Nutt.* (Putty Root.)
hyemale, *Nutt.*
Orchis, *Linn.*
spectabilis, *Linn.*
Platanthera, *Richards.*
orbiculata, *Lindl.*
Hookeri, *Lindl.*
bracteata, *Torr.*
hyperborea, *Lindl.*
leucophæa, *Nutt.*
psycodes, *Gray.* (O. fimbriata, *Pursh.*)
Pogonia, *Juss.*
ophioglossoides, *Nutt.* Waukesha, *Mr. G. H. Cornwall.*
Calopogon, *R. Brown.*
pulchellus, *R. Br.*
Spiranthes, *Richards.* (Lady's Tresses.)
gracilis, *Bigelow.*
cernua, *Richards.*
Goodyera, *R. Brown.*
pubescens, *R. Brown.*
Cypripedium, *Linn.* (Lady's Slipper.)
pubescens, *Willd.* (Yellow Lady's Slipper.)
parviflorum, *Salisb.* (Small Yellow Lady's Slipper.)
candidum, *Muhl.* (White Lady's Slipper.)
spectabile, *Swartz.* (Moccason Flower.)
acaule, *Ait.* (Purple Lady's Slipper.)

AMARYLLIDACEÆ. (The Amaryllis Family.)

Hypoxis, *Linn.* (Star Grass.)
erecta, *Linn.*

IRIDACEÆ. (The Iris Family.)

Iris, *Linn.*
versicolor, *Linn.* (Blue Flag.)
lacustris, *Nutt.*
Sisyrinchium, *Linn.* (Blue-eyed Grass.)
Bermudianum, *Linn.*

DIOSCORACEÆ. (The Yam Family.)

Dioscorea, *Plumier.*
villosa, *Linn.*

SMILACEÆ. (The Green Brier Family.)

§ 1. *Smilaceæ.*

Smilax, *Tourn.*
rotundifolia, *Linn.* (Green Brier.)
herbacea, *Linn.*
lasioneuron, *Hook.*

§ 2. *Trilliaceæ.*

Trillium, *Linn.*
cernuum, *Linn.* (T. pendulum, *Muhl.*)
grandiflorum, *Salisb.*
nivale, *Riddell.*
recurvatum, *Beck.*
sessile, *Linn.*
Medeola, *Gronov.*
Virginica, *Linn.*

LILIACEÆ. (The Lily Family.)

§ 1. *Asparageæ.*

Asparagus, *Linn.*
officinale, *Linn.* (Introduced.)
Polygonatum, *Tourn.* (Solomon's Seal.)
pubescens, *Pursh.*
Smilacina, *Desf.*
racemosa, *Desf.*
stellata, *Desf.*
trifolia, *Desf.*
bifolia, *Ker.*
Clintonia, *Raf.*
borealis, *Raf.*

§ 2. *Asphodeleæ.*

ALLIUM, *Linn.* (Garlic.)
Canadense, *Kalm.*
cernuum, *Roth.*
tricoccum, *Ait.* (Leek.)

§ 3. *Tulipaceæ.*

LILIUM, *Linn.* (Lily.)
Philadelphicum, *Linn.* (Orange Lily.)
Canadense, *Linn.* (Nodding Lily.)
ERYTHRONIUM, *Linn.* (Dog's Tooth Violet.)
Americanum, *Smith.*
albidum, *Nutt.*

MELANTHACEÆ. (THE COLCHICUM FAMILY.)

§ 1. *Uvularieæ.*

UVULARIA, *Linn.* (Bellwort.)
grandiflora, *Smith.*
STREPTOPUS, *Michx.*
roseus, *Michx.*

§ 2. *Melanthieæ.*

ZYGADENUS, *Michx.*
glaucus, *Nutt.*
TOFIELDIA, *Hudson.*
glutinosa, *Willd.*

JUNCACEÆ. (THE RUSH FAMILY.)

LUZULA, *DC.* (Woodrush.)
pilosa, *Willd.*
campestris, *Linn.*
JUNCUS, *Linn.* (Rush.)
Balticus, *Willd.*
scirpoides, *Lam.* (T. polycephalus, *Mx.*)
acuminatus, *Michx.*
tenuis, *Willd.*

PONTEDERIACEÆ. (THE PICKEREL-WEED FAMILY.)

PONTEDERIA, *Linn.* (Pickerel-Weed.)
cordata, *Linn.*

COMMELYNACEÆ. (The Spiderwort Family.)

Tradescantia, *Linn.* (Spiderwort.)
  Virginica, *Linn.*

CYPERACEÆ. (The Sedge Family.)

Cyperus, *Linn.*
  diandrus, *Torr.*
  inflexus, *Muhl.* Bass Lake, Dane Co. *I. A. Lapham.*
  strigosus, *Linn.*
  filiculmis, *Vahl.* Upper Mississippi, *Dr. Houghton.*
Dulichium, *Richard.*
  spathaceum, *Pers.*
Eleocharis, *R. Brown.*
  obtusa, *Schultz.*
  palustris, *R. Brown.*
  tenuis, *Schultz.*
  acicularis, *R. Brown.*
Scirpus, *Linn.*
  pungens, *Vahl.* (S. triqueter, *Mx.*)
  lacustris, *Linn.* (Bulrush.)
  fluviatilis, *Gray.* (S. maratimus, var. *Torr.*)
  atrovirens, *Muhl.*
  lineatus, *Michx.*
  Eriophorum, *Michx.*
Eriophorum, *Linn.* (Cotton Grass.)
  alpinum, *Linn.* Lake Superior, *Dr. Houghton.*
  vaginatum, *Linn.*
  Virginicum, *Linn.*
  polystachyum, *Linn.*
  angustifolium, *Richard.*
Carex, *Linn.* (Ledge.)
  aurea, *Nutt.*
  anceps, *Willd.*
  bromoides, *Schk.*
  Buxbaumii, *Wahl.*
  bullata, *Schk.*
  chordorrhiza, *Ehrh.*
  comosa, *Boott.*
  Deweyana, *Schw.*
  eburnea, *Boott.*

(Carex)

festucacea, *Schk.*
gracilis, *Ehrh.* (C. disperma, *Dewey.*)
granularis, *Muhl.*
gracillima, *Schw.*
hystriciana, *Willd.*
intermedia, *Good.*
irrigua, *Smith.*
intumescens, *Rudge.*
laxiflora, *Lam.*
lanuginosa, *Michx.* (C. pellita, *Muhl.*)
lacustris, *Willd.*
lupulina, *Muhl.*
longirostris, *Torr.* Blue Mounds, *I. A. Lapham.*
mirabilis, *Dew.*
oligosperma, *Michx.*
polytrichoides, *Muhl.*
panicea, *Linn.* (C. Meadii, *Dew.*)
Pennsylvanica, *Lam.*
pubescens, *Muhl.* Brookfield, *Mr. M. Spears.*
rosea, *Schk.*
rigida, *Good.* (C. saxatilis.) Lake Superior, *Dr. Houghton.*
stipata, *Muhl.*
sparganoides, *Muhl.*
stellulata, *Good.*
straminea, *Schk.*
stricta, *Lam.* (C. acuta, *Muhl.* C. angustata, *Boott.*)
teretiuscula, *Good.*
tenera, *Dew.*
vulpinoidea, *Michx.* (C. multiflora, *Muhl.*)

GRAMINEÆ. (The Grass Family.)

Leersia, *Solander.* (White Grass.)
oryzoides, *Swartz.* (Cut Grass.)
Virginica, *Willd.* (White Grass.)
Zizania, *Gronov.* (Wild Rice.)
aquatica, *Linn.*
Alopecurus, *Linn.* (Fox-Tail Grass.)
geniculatus, *Linn.*
Phleum, *Linn.* (Timothy.)
pratense, *Linn.*

AGROSTIS, *Linn.* (Bent-Grass.)
scabra, *Willd.* (Thin Grass.) (A. Michauxii, *Trin.*)
vulgaris, *With.* (Redtop.)
CINNA, *Linn.*
arundinacea, *Linn.*
MUHLENBERGIA, *Schreber.*
glomerata, *Trin.* (Polypogon racemosa, *Nutt.*)
Mexicana, *Trin.* (Agrostis lateriflora, *Mx.*)
Willdenovii, *Trin.* (Agrostis tenuiflora, *Willd.*)
BRACHYELYTRUM, *Beauv.*
aristatum, *Beauv.* (Muhlenbergia erecta, *Schreb.*)
CALAMAGROSTIS, *Adans.*
Canadensis, *Beauv.*
ORYZOPSIS, *Michx.* (Mountain Rice.)
asperifolia, *Michx.*
melanocarpa, *Muhl.* (Piptatherum nigrum, *Torr.*)
STIPA, *Linn.*
avenacea, *Linn.*
SPARTINA, *Schreb.* (Cord Grass.)
cynosuroides, *Willd.*
BOUTELOUA, *Lagasca.* (Atheropogon, *Muhl.*)
racemosa, *Lag.* (A. apludoides, *Michx.*)
papillosa, *Gray.* Cassville, *Dr. Houghton.*
KŒLERIA, *Pers.*
cristata, *Pers.* (K. nitida, *Nutt.*)
REBOULEA, *Kunth.*
obtusata, (Kœleria truncata, *Torr.*)
GLYCERIA, *R. Brown.*
Canadensis, *Trin.* (Poa Canadensis.)
nervata, *Trin.* (Poa nervata, *Willd.*)
fluitans, *R. Brown.*
POA, *Linn.* (Meadow Grass.)
debilis, *Torr.*
nemoralis, *Linn.*
serotina, *Ehrh.*
trivialis, *Linn.*
pratensis, *Linn.*
ERAGROSTIS, *Beauv.*
megastachya, *Link.* (Poa eragrostis, *Linn.*)
FESTUCA, *Linn.*
nutans, *Willd.*

BROMUS, *Linn.* (Brome Grass.)
ciliatus, *Linn.*
purgans, *Linn.*
secalinus, *Linn.* (Chess.)
PHRAGMITES, *Trin.* (Reed.)
communis, *Trin.*
TRITICUM, *Linn.* (Wheat.)
repens, *Linn.* (Couch-Grass.)
ELYMUS, *Linn.* (Lime-Grass.)
Virginicus, *Linn.*
Canadensis, *Linn.*
glaucifolius, *Muhl.*
striatus, *Willd.*
hystrix, *Linn.* (Bottle-Brush Grass.)
HORDEUM, *Linn.* (Barley.)
jubatum, *Linn.* (Squirrel-Tail Grass.)
AIRA, *Linn.* (Hair-Grass.)
cæspitosa, *Linn.*
DANTHONIA, *DC.* (Wild-Oat Grass.)
spicata, *Beauv.*
AVENA, *Linn.* (Oat.)
striata, *Michx.* (Trisetum purpurascens, *Torr.*)
HOLCUS, *Linn.* (Velvet Grass.)
lanatus, *Linn.* Upper Mississippi, *Dr. Houghton.*
HIEROCHLOA, *Gmelin.* (Seneca Grass.)
borealis, *Rœm. & Sch.*
MILIUM, *Linn.* (Millet-Grass.)
effusum, *Linn.*
PANICUM, *Linn.* (Panic-Grass.)
capillare, *Linn.*
virgatum, *Linn.*
latifolium, *Linn.*
clandestinum, *Linn.*
dichotomum, *Linn.*
pubescens, *Lam.*
crus-galli, *Linn.* (Barn-Yard Grass.)
longisetum, *Torr.* Neenah River, *Prof. Douglass.*
SETARIA, *Beauv.*
glauca, *Beauv.*
CENCHRUS, *Linn.* (Burr-Grass.)
tribuloides, *Linn.* Upper Mississippi, *Dr. Houghton.*

ANDROPOGON, *Linn.* (Beard-Grass.)
furcatus, *Muhl.*
scoparius, *Michx.*
SORGHUM, *Pers.*
nutans. (Andropogon nutans, *Linn.*)

EQUISETACEÆ. (THE HORSE-TAIL FAMILY.)

EQUISETUM, *Linn.* (Horse-Tail.)
arvense, *Linn.*
eburnum, *Schreb.* Lake Superior, *Dr. Torrey.*
sylvaticum, *Linn.*
limosum, *Linn.*
hyemale, *Linn.* (Scouring Rush.)
lævigatum, *Braun.*
variegatum, *Schleicher.*

FILICES. (THE FERN FAMILY.)

POLYPODIUM, *Linn.*
vulgare, *Linn.* Blue Mounds, *I. A. Lapham.*
STRUTHIOPTERIS, *Willd.* (Ostrich Fern.)
Germanica, *Willd.* Lake Superior, *Dr. Houghton.*
ADIANTUM, *Linn.* (Maiden Hair.)
pedatum, *Linn.*
PTERIS, *Linn.* (Brake.)
aquilina, *Linn.*
atropurpurea, *Linn.*
CAMPTOSORUS, *Link.* (Asplenum, *Linn.*)
rhizophyllus, *Link.* (Walking Leaf.)
ASPLENIUM, *Linn.* (Spleenwort.)
thelypteroides, *Michx.*
Filix-femina, *R. Brown.* (Aspidium asplenoides.)
CYSTOPTERIS, *Bernhardi.* (Bladder Fern.)
bulbifera, *Bernh.*
WOODSIA, *R. Brown.*
ilvensis, *R. Brown.* Blue Mounds, *I. A. Lapham.*
DRYOPTERIS, *Adans.* (Aspidium, *Linn.*) (Wood Fern.)
thelypteris, *Gray.*
cristata, *Gray.*
Goldiana, *Gray.*

ONOCLEA, *Linn.* (Sensitive Fern.)
sensibilis, *Linn.*
OSMUNDA, *Linn.* (Flowering Fern.)
spectabilis, *Willd.*
Claytoniana, *Linn.* (O. interrupta, *Mx.*)
cinnamomea, *Linn.*
BOTRYCHIUM, *Swartz.*
lunarioides, *Sw.* (B. fumarioides, *Willd.*)
Virginicum, *Sw.*

LYCOPODIACEÆ. (THE CLUB-MOSS FAMILY.)

LYCOPODIUM, *Linn.* (Club-Moss.)
lucidulum, *Michx.*
annotinum, *Linn.* Lake Superior, *Dr. Houghton.*
dendroideum, *Michx.* Lake Superior to Upper Mississippi, *Dr. Houghton.*
clavatum, *Linn.*
complanatum, *Linn.*
SELAGINELLA, *Beauv.*
rupestris, *Spring.* (Lyc. rupestre, *Linn.*) Blue Mounds.
apus, *Spring.* (L. apodum, *Linn.*)

CHARACEÆ. (THE CHARA FAMILY.)

CHARA, *Linn.*
Vulgaris, *Willd.* (Feather-Beds.)

The fourth paper, communicated by Prof. AGASSIZ, in the absence of the author, is

A LIST OF THE FOSSIL CRINOIDS OF TENNESSEE. BY PROF. G. TROOST, of Nashville.

ECHINITES.

CIDARITES Tennesseæ *mihi.*
MELONITES multipora, *Norwood & Owen.*

ASTERIAS.

ASTERIAS antiqua* *mihi.*
ASTRIOS Tennesseæ.

* I described an Asterias under this name, which was published in 1834 in the Transactions of the Geological Society of Pennsylvania. Hisinger published in 1837 a description under the same name.

CRINOIDEA.

Campanulites tessellatus *mihi*.
Catillocrinites Tennesseæ.
Caryocrinites meconideus.
hexagonus.
granulatus.
insculptus.
globosus.
Pentremites pyriformis, *Say*.
Tennesseæ *mihi*.
florealis, *Say*.
elongatus var.
Cherokeus *mihi*.
Reinwardtii.
Cacabocrinites sculptus.
Codonocrinites gracilis.
Echinoencrinites fenestratus.
Actinocrinites moniliformis, *Miller*.
Humboltii *mihi*.
gibbosus.
Agassizii.
Urna.
Nashvillæ.
cornutus.
Fibula.
Verneulii.
Mortoni.
Balanocrinites sculptus.
Heterocrinites simplex, *Hall*.
Agaricocrinites tuberosus *mihi*.
Conocrinites tuberculosus *mihi*.
Leæ.
Eucalyptocrinites splendidus.
ovalis.
extensus.
lævis.
Phillipsii.
Goldfussii.
Nashvillæ.

(EUCALYPTOCRINITES)
conicus.
Tennesseæ.
gibbosus.
GILBERTSOCRINITES americanus.
CYATHOCRINITES inflatus.
stellatus.
gracilis.
corrugatus.
Tennesseæ.
planus ? *Miller*.
robustus *mihi*.
crateriformis.
globosus.
depressus.
tiaræformis.
sculptus.
conglobatus.
Römerii.
ZEÆCRINITES magnoliæformis.
POTERIOCRINITES municipalis.
SYNBATHOCRINITES Tennesseæ.
granulatus.
CUPELLÆCRINITES Verneuilii.
lævis.
striatus.
Buchii.
magnificus.
corrugatus.
stellatus.
rosæformis.
pentagonalis.
inflatus.
HAPLOCRINITES hemisphæricus.
ovalis.
granulatus.
maximus.
PLATYCRINITES Ann Dixoni.
Huntsvillæ.

(PLATYCRINITES)

polydactylus.

insculptus.

DONACICRINITES simplex.

DÆMONOCRINITES cornutus.

ASTYLOCRINITES.

CRUMENÆCRINITES ovales *mihi*.

AGASSIZOCRINITES dactilyformis.

gracilis.

GRANATOCRINITES cidariformis.

OLIVANITES Verneuilii.

globosus.

There are sixteen new genera. The six last are without column, all silurian.

Prof. AGASSIZ, in offering the above, remarked:

This is merely a nominal list, so that I have no means of ascertaining how far these are all distinct species; but even upon the supposition that, upon slight distinctions, there have been too many distinct species named, which upon final examination shall be combined again, it shows a wonderful development of that form of animals during the silurian and carboniferous periods in the west; and no doubt a full illustration, with a good description and correct figures of all these animals, would throw much light upon a type of the animal kingdom, of which in the present age, we have but one dwarfish and one handsome species left, but which, to the almost total exclusion of other animals of the class echinoderms, once occupied the ancient shores during the palæozoic period. Here are thirty-one genera mentioned, sixteen of which are considered by Prof. Troost as new. Here are a great many species, not less than eighty-six in number, of which only half a dozen are marked as already described. Now my friend, Prof. Hall, tells me that this list, if it is correct in all its parts, is greater than the number of species which occur in the State of New York, and from the species which have already been noticed publicly, no doubt it is as correct as such lists can be expected to be, and a full illustration of them would be a great addition to the stock of our knowledge of fossils. It is the more desirable that all these fossils should be made known, as the family of crinoids is so reduced

in our days that we can form no adequate idea of the animals of that group, of their diversity of form, modifications of character, and peculiarity of position, from the living type only.

The study of fossil crinoids in another point of view is highly important. They were first arranged among the polypi. They were afterwards discovered to be star-fishes provided with a stem, and thus expanded our ideas of zoölogical relations. Since the number of known fossil species is increased, and the number of forms enlarged, it is found that these forms of crinoids of former ages foretell, as it were, the diversity which this class has undergone in more recent periods, aping at earlier periods the forms of later ages, their peculiar condition of existence, &c., so that in every respect a monograph would be a highly valuable addition to our knowledge; and I am happy to be able to state that Prof. Troost is preparing a monograph of all these crinoids, illustrated by 220 figures. The peculiarities upon which the new genera rest, no doubt, are modifications of structure unnoticed before, as he has placed the genera described before in such an order as will justify the supposition, that the new types introduced between them differ really from the others already well known. There can be no doubt that very many of these peculiarities have never been observed before.

Prof. Hall. I would remark, that the number of species here enumerated (and I presume it is correct) as being found in Tennessee, is greater than all we yet know. In the State of New York, I have ascertained the characters of something more than 40 species — 27 of which are in a single group, the Niagara limestone. In Tennessee, I understand, the crinoids are extremely abundant, since Dr. Rœmer states that he has been able to collect some 300 or 400 good specimens in a morning, of 7 or 8 different species, in localities which he said he presumed no other person had visited; and I can therefore very readily believe that he has observed very many distinct species heretofore undescribed. I presume that all the formations of New York will not afford more than 60 species — 27 of which have been found in a limited space not exceeding 100 feet in thickness. I have seen 8 or 10 species from the west, entirely different from those in New York. I presume, therefore, that Prof. Troost has not only the number he has enumerated, but perhaps a great many more. The number of species that were known in the State of New York, previous to the beginning of the geological survey, did

not exceed the number, I believe, of 4 or 5, and now we have pretty well ascertained some 40 species; and it is not surprising that at the west there should be a great many new species. Such a monograph as Prof. Troost is preparing will be very valuable. I mentioned the fact some time since, that all the crinoids in the lower Silurian Rocks, with the exception of one species, have five pelvic plates, and that we never find one with three, or any other number of these plates, before we reach the higher deposits. I have one, from the upper part of the Hamilton group, that has six pelvic plates.

Prof. Agassiz. It is, perhaps, not sufficiently appreciated of what importance, and of what immense value, the study of these fossils may be for the progress of Palæontology, and American students should be proud of these materials by which they will be able to throw so much light upon these almost extinct families by their personal investigations, which will not only render them independent of the Palæontologist from abroad for information with regard to the succession of types, and the full illustration of these structures, but really afford correct standards for comparison. I doubt whether the number of crinoid heads of all species found in Europe, now existing in the museums of Europe, is one-third the number of those which Professor Hall has just mentioned as having been found one morning in Tennessee, by a single gentleman. Now with such materials, consider what precise and what minute investigations could be made. And if these facts can be once fully ascertained and well illustrated, I have no doubt that the series of crinoids, and their succession in former ages, will be established from American standards, and will no longer rest upon the European evidence, which has often been derived from the examination of few fragments of those ancient fossils, found in unconnected basins for the most part, so that their geological succession could be ascertained only with great doubt and difficulty. And I venture to say, that geologists who have had any opportunity to compare the position of the ancient rocks on this continent, with the corresponding deposits in Europe, will agree with me, when I say that the geology proper, the stratography of this continent, will afford the same precise and well authenticated standards for the appreciation of the order of succession of rocks, as fossils will for the order of succession of living beings during the earliest period of the history of our globe.

The fifth paper was offered by the Secretary, namely —

A Plan for the Diffusion of Human Knowledge. By Rev. Roswell Park.

Prof. Horsford stated that the plan contemplated the introduction of uniform standard books for elementary and collegiate education, and suggested the consideration of this subject by the Association. Upon motion, the paper was referred to the Standing Committee.

The sixth paper was as follows:

Correspondence in Relation to the Altona Observatory.

Extracts from diplomatic letters, relative to the Observatory of Altona, were read to the Association, by B. A. Gould, Jr., of Cambridge. This correspondence had been given by Mr. Clayton, Secretary of State, to the President of the Association, with permission to have it communicated to the meeting. Mr. Gould, before reading the papers, alluded to the gratifying intelligence they contained, that Prof. Schumacher would, in all probability, be soon relieved from the embarrassing situation in which he has been placed for the last two years, in consequence of the unfortunate hostilities in the Duchies of Holstein and Schleswig, in the former of which the Observatory is situated. That he should be relieved as speedily as possible, was a matter of deep interest, not only to all interested in the progress of astronomical science, but to all who loved science at all, and felt in its behalf grateful to one whose eminent talents, profound learning, and unceasing labors, had done so much to promote it. The only purely astronomical journal of a high order published in the world, was the Astronomische Nachrichten of Prof. Schumacher, a journal which had done more to break away the petty barriers of nationality in science, than any thing else of the kind. It was the focus to which the astronomers of the whole civilized world sent their most striking results for immediate publication, — the position and character of its editor enabling him to serve as a connecting link between the mathematicians and astronomers of the several countries of Europe and America. Its existence was invaluable; but the distinguished astronomer who conducts it has been reduced to the necessity of either suspending it entirely, or taking the burden of its support upon himself. Prof. Schumacher has nobly done the latter, and not only this; he has borne the whole burden of the Altona Observatory also, an Observatory which was founded for him, by the liberality of the late

kings of Denmark, and which he has raised to the reputation of an Observatory of the first class.

The debt which the whole astronomical world owes this illustrious scientist is too great to allow the learned societies of any country to remain silent while he was suffering under such embarrassment, — and the academies of science, and philosophical societies of all civilized countries had, with one voice, protested, in the name of Science, and urged their respective governments to interfere. Our own scientific institutions had represented the urgency of the case to the government of the United States. That government had promptly responded to the call, by addressing itself both to the Danish and to the Schleswig Holstein authorities. The papers which Mr. Gould was about to read would show the success of this intervention.

Extract from Mr. Bancroft's Despatch to the Secretary of State.

" The President has expressed his interest in the safety and prosperity of the illustrious Astronomer Schumacher. I have seen, on the subject, the envoy of Germany, and a member of the late Provisional Government of Schleswig Holstein. Both manifested towards me the utmost cordiality on the occasion, and the latter authorized me to say to the President that his friendly interest in the affairs of the Altona Observatory was esteemed an honor to his country, and that the Schleswig Holstein Government had already resolved to sustain the Observatory, and to secure to Mr. Schumacher all his appointments.

I remain, Sir, sincerely yours,
(Signed,) GEORGE BANCROFT."

Translation of Extracts from a Note addressed to the Baron of Ungern-Steinberg and to Sir Henry Wynn, the Russian and English Ministers at Copenhagen.

" The Observatory of Altona, established by the regal munificence of Frederick IV. and of Christian VIII., is placed, in consequence of the insurrection which has broken out in the Duchies of Holstein and Schleswig, in a position by no means conformable to the intentions of its august founder, nor compatible with the interests of the scientific world, — which demands the preservation of an establishment that has served, under the learned direction of Mr. Schumacher, as a centre for great scientific enterprises. You, sir, have supported these interests in pressing upon the government of the king your good offi-

ces in behalf of Mr. Schumacher ; and in expressing the desire that this distinguished astronomer should be maintained for life in his position as established by king Christian VIII.

"You will not doubt the serious attention which has been devoted to this affair. In the midst of the painful events which afflict her, Denmark has always remained faithful to the great scientific interests which have been entrusted to her care. She will show this, too, in the present circumstances."

. . . .

"But being deprived of the exercise of his legitimate authority in the Duchies, and being unable to foresee the development of the future relations between Holstein and the kingdom of Denmark, properly so called, — the government of the king ought, doubtless, to feel a proper hesitancy as to continuing under such circumstances the expenditure of the large sums necessary for maintaining the accustomed activity of Mr. Schumacher, without possessing in return the least guaranty that the ownership of the collections of instruments in the Observatory of Altona shall be preserved inviolate to the state.

"Upon the proposal of the Minister of Finance, the Council of State has resolved, — that the sum allowed for the support of the Observatory of Altona, shall be forwarded to Mr. Schumacher as soon as an effectual guaranty shall have been obtained, — that the said collections of instruments used by Mr. Schumacher, at the expense of the king's government, shall always remain the property of the state, and be recognized as such, — or that the value of these collections should be, at all events, restored to the Danish government, should the Altona Observatory pass into other hands.

"Permit me, sir, to call for your kind intervention, that such a guaranty may be afforded the government of the king, both now and hereafter, in a separate article of the peace which shall be concluded between Denmark and Germany, — and have the goodness to inform me of the result of the steps which you shall judge proper to be taken, as regards your government, for this end. Receive, &c.

(Signed,) MOLTKE."

President EVERETT stated that, having had the honor to act as the medium of communication between Prof. Schumacher and the men of Science of America on this occasion, and to serve upon the Committee which had been raised on this subject by the American Academy of Arts and Sciences, he had received a short time since a letter

from M. Steen Bille, the Danish Chargé d'Affaires at Washington, transmitting a copy of the communication from the Danish Minister of Foreign Affairs to the English and Russian Ministers at Copenhagen. As that communication had just been read by Mr. Gould, Mr. Everett would only read the short letter of M. Steen Bille accompanying it.

Having read M. Steen Bille's letter, Mr. EVERETT observed, that among the late items of intelligence from Europe, it was well known that it was stated that preliminaries of a treaty had been concluded between Denmark and the Duchies. Of its definitive ratification we had not yet heard; but he supposed there was no doubt that it would take place. There was reason, therefore, to feel confident that, in due time, a satisfactory arrangement would be made in reference to Mr. Schumacher's position and the Observatory at Altona. This was a most gratifying piece of intelligence, inasmuch as the offer of the Danish government was attended with conditions, which, however reasonable in themselves, it would have been extremely difficult, while the war lasted, to carry into effect.

Prof. HENRY remarked that the communication had been sent to him by the Secretary of State, with permission to lay it before the Association, and it had therefore been offered by Dr. Gould, at his request.

ON THE STRUCTURE OF CORAL ANIMALS. By Prof. LOUIS AGASSIZ.

During an excursion in the Vineyard Sound with Lieut. Davis, in one of the Coast Survey steamers, about six weeks ago, it was my good fortune to bring up in the dredge, from the depth of 72 feet, off Gay Head, several specimens of a coral with its animals, which I succeeded in preserving alive for several days, upon which I made very careful observations, and from which I had drawings made of the details of its microscopic structure. After the investigation was finished, I tried to preserve the animal some time longer, with the hope of presenting it for the examination of my friends; and I therefore continued the care I had bestowed upon it, changing the sea-water twice a day; and it has been my good fortune to keep them alive for the past six weeks; and I now present you, in this jar, *live corals* from the coast of Massachusetts, and which I will request you to step forward and examine.

[After a few minutes examination of them, by the audience, one of

the specimens being completely opened, with the long and slender tentacles of the living inhabitants entirely out, Prof. A. proceeded with his illustration.]

With your permission, I will now proceed to detail the structure of this animal. The naturalists who have traced the progress of our knowledge of this class of animals, must be aware that Mr. James Dana, of New Haven, in his illustrations of the Polypi collected during the Exploring Expedition, has presented the state of our knowledge of this class, as it has really been obtained by all former investigations, adding from his own observations, invaluable information, and illustrating all the types of this class in a most splendid manner, unsurpassed by any former observer. The travelling naturalist being, however, scarcely ever in the position of making prolonged microscopical observations, has left me a chance for gleaning in this field; and the fact that I have had these animals so many weeks alive before me has enabled me to add some remarks upon its microscopic structure, which are of some interest. I will add, at the outset, that these investigations have enabled me to satisfy myself fully of the correctness of Mr. Dana as to his classification of these animals. All authors who have attempted to classify the Polypi, before the publication of Mr. Dana's work, distinguish the soft types, with their varying forms, as a peculiar group, distinct from all others, and then sub-divide the coral-building Polypi according to their structure into many sub-divisions. No one having attempted among them what Cuvier did among the Molluscs, to bring together those possessing hard parts, and those deprived of them, upon common characters of structure. Indeed, naked Polypi have always been considered distinct in their structure from those building corals; but Mr. Dana in his researches, assisted by Dr. Wyman and Dr. Gould, in the examination of the Actinia, came to the conclusion from what he saw of these and of the coral-building Polypi, that they must all be brought into great divisions, according to their structure; and he actually united the Actinia with the true Madreporidæ.

I would resume my observations upon this Astroid Polyp, as it belongs to the great family of Astrea, by saying, in one word, that it is a diminutive Actinia, and that we have all the details of structure which Actinia present, in a miniature form, in Astrea. My animal, however, belongs strictly to the genus Astrangia, and I would propose to dedicate the species to the able describer and illustrator of the Polypi of the United States Exploring Expedition, and call it in future *Astrangia Danæ*.

This species presents two varieties. Some specimens are of a pink or rosy color, others are white. The highly colored specimens are rather faded away, but I shall circulate drawings representing these animals as they appeared several weeks ago. The general form of the animal is a cylinder, (as of all the Polypi,) resting upon its base, and expanded on the upper margin. Thus expanded it is about two lines in diameter. The number of tentacles is definite, but it is not always the same absolute number. It never exceeds 24, but in earlier periods of life there are only 12, and there is even an epoch of life when there are only six; which shows plainly that the number of tentacles is always a multiple of six. Seen from above, these tentacles are placed in the following position, which is constant. Just in the line of the longest diameter of the mouth, — which, by the way, is never circular in Polypi, but always oblong, thus introducing somewhat of the bilateral symmetry,—just in that line are placed two of the six larger tentacles, and at equal distances, in a circle are four more. This circle, containing six, is the first formed. These grow larger as the animal grows, and presently another row is introduced, alternating with the former, and smaller, making 12 tentacles. And finally a third row alternates with the others, thus constituting three rows, of gradually smaller tentacles, 24 in number. I have never seen any grow beyond this number; but I have seen young ones forming, since I have had them in my possession, and beginning with six; I have seen some with only 12; but the greater part of them have 24. I have never seen any of them pass into the state in which they have 24, but I have occasionally seen those which at one time had only six, to have afterwards 12; and I have seen those which at one time had only 12, to have afterwards 24. It is very difficult to watch the whole process of these changes, although I have been able to ascertain how they take place, and to determine the relative position of these tentacles to the cavity of the body and to the mouth. They move slowly; the motions resembling those of the tentacles of the snails.

We have, below the mouth, a small cavity, which is shut underneath by the contraction of its walls, and which, immediately below, expands into a wider cavity. The upper cavity is the stomach. In the centre of it is a large opening, which communicates with the cavity below; so that the stomach, by the relaxation of its walls, that is, of its muscular fibres, throws down its own contents into the general cavity of the body. But during the process of digestion, when food

has been introduced into this cavity — for I have fed these animals with fragments of the animal of various kinds of shells, and thus been able to trace the progress of digestion — the mouth is shut, and the stomach is equally shut below. During the whole time that digestion goes on, the stomach remains as a closed bag; but as soon as the food has been fully digested, then the lower hole opens, to empty its contents into the general cavity; but sometimes the upper opening expands first, and the refuse, of hard particles, is thrown away. The lower opening of the stomach is shut again as soon as the homogeneous mass of the digested food has entered into the wide cavity below, precisely as in Actinia; so much so that you might imagine I was describing such polyps, though I am describing an animal entirely different in its external aspect.

The general cavity of the body presents the following peculiarities: The walls of the body forming this cavity are provided, at the inner margin, with partitions advancing inside. These walls reach as high as the lower opening of the stomach, and extend also to the sides of the stomach, forming complete partitions between the stomach and the walls in the upper portion of the animal, but only half partitions in the lower part of the animal; so that the partitions communicate with each other in this region. This cavity contains always water, in greater or less quantities, which is introduced into it through the stomach chiefly, and probably, also, through other apertures, and it may be through the whole surface of the body, as in Actinia; but here I could not satisfy myself about the facts. Through the mouth, then, a certain quantity of water is constantly introduced into the body, which is expanded by this water. The food, now digested, is mixed with it, and so prepared for circulation. This mixture of water and digested food is set in motion by the surface of all these walls, and I will now explain the mechanical apparatus for putting in motion all these materials. The fact is, that all the surfaces, in all these cavities, are provided with vibrating cilia, which establish at once a regular circulation of the liquid between the partitions and up into the closed cavities above; and so the food, diluted with water, is brought to all parts of the body.

The hard parts of the Polypi are formed, within the wall, in the thickness of the wall of the animal itself. The hard part of the Polypi, Mr. Dana has already shown to us, are neither an external secretion, nor an inner skeleton. They constitute a calcareous deposit within the soft parts of the animal itself. In order that this may

be understood, I will now proceed to give some illustrations of the microscopic structure of the tissue itself. If we suppose a cut across the wall, representing the thickness of the wall, we have at the surface lining the inner cavity, a layer of cells. We have also a layer of cells outside. Now every one of these cells is a vibrating cell, the whole of its margin being provided with cilia. If one of these cells is detached and examined separately, it is found to be something of a bell form, flat outwards, and its whole margin provided with vibratory cilia, which, being put in motion, cause the currents of the water in the inner cavities. A similar apparatus on the outer surface of the body determines the currents by which the surrounding water is renewed, and minute particles of food are brought around and into the mouth, and thus into the stomach. Between these internal and external layers of cells, there are other cells generally very much elongated longitudinally, and these cells are the muscular fibres between the two layers of vibrating cells. Now it is only in the lower portion of the wall, below the layer of muscular fibres, and never in its upper tubular parts, that the secretion of limestone takes place. It consists of small granules of limestone, accumulating in the base and lower throughout cellular part of the walls of the body, resembling in their aggregation stone walls combined in different ways. It is thus by the accumulation of microscopic granules of limestone, formed in cells, that a regular wall of limestone is produced, within the thickness of the membrane itself, in its lower portion, and enclosed in it. Placed under the microscope, such a portion of coral is seen to consist entirely of little granules, agglutinated in the same manner as it is commonly seen that limestone particles resting upon each other may unite.

In this type it is curious to see how the growth takes place; and I will now proceed to illustrate this point. If we consider from the outside the wall between two inner partitions, we may detect a new individual forming simply by the projection of the wall in the shape of an hernia. By its contraction, the whole surface is wrinkled, and the wall, instead of being straight, or smooth, becomes undulated. One of these prominences projecting more than the others, may remain as a diverticulum, grow larger, and become a prominent tubercle. After eight or ten hours, there is a very visible tubercle, of the size of a pin's head, and this is the beginning of a young polypus. It will increase, and, within forty-eight hours, there will be a little opening forming at the end, so that it is beginning to

have a cavity. Upon the outside surface, then the buds begin to form, first as small sacks, and, after less than seven days, I have seen,— by this process of *sacking out*, as it were, of elongating tubercles — a new polyp entirely developed with six tentacles, but without any particle of solid matter in it. At the beginning it is entirely soft, as the Actinia, forming a sort of bag upon the side of the mother Polyp; this, however, takes place only at the lower part of the body.

But these animals increase also in another manner. The membrane at the base will expand beyond its usual limits, and deposit a flat layer of calcareous matter below, and the membrane, thus started, will grow upwards, and form a prominent bag, which finally opens, and gives rise to another polyp on the side of those already developed. So that there are two very distinct modes of reproduction in these animals. I have not been able to trace how the lateral soft buds assume solid parts afterwards. It may be that they separate by contraction, and attach themselves at some distant point, to form a new colony, as we know it to be the case with Hydra. Those which rise at once from a limestone foundation, grow from the base of the mother stem.

But this is not the whole amount of structural details which have been observed in these animals. I have already mentioned the tentacula as presenting a sluggish motion, and as having vibrating cilia upon their surface to determine currents, by which very minute particles of food are brought to their mouth. The structure of these small tentacles is, however, so complicated as to require further illustration, to give a full account of their action. They are hollow, as I have mentioned, and their inner surface is also lined with vibrating cilia, which form a kind of festoon all over the inner surface, with vibrating fringes all the way along. There are muscular fibres, longitudinal as well as circular, between the inner and outer layer of epithelial cells; but there are upon the external surface, besides the vibrating cilia, peculiar organs which have been known to exist in other animals, but which have never before been observed in corals, called nettling organs. It is very well known that the jelly fishes, if handled, leave a painful sensation like that of the burning of nettles. It has been ascertained that this nettling arises from the action of a peculiar apparatus, having the form of slender thread issuing from a bulb. Now, in this coral animal, almost the whole surface of the tentacle is provided with heaps of such nettling apparatus, arranged all over the surface like warts, nearly in rows. There are hundreds of

these warts upon one of these tentacles, and if we examine their structure under high powers, we find that every wart consists of numerous nettling cells. The whole structure of these cells can scarcely be fully recognized by the best microscopes now at our disposal. Some peculiarities which I succeeded in seeing one day, escaped my observation the next; and it was only by repeated observations, after trying several microscopes, that I satisfied myself that I had seen correctly what I shall now describe. Even some of the microscopes, considered among the best, do not reach the limits which are required for such investigations. These heaps of wartlike bodies are accumulations of peculiar cells, and there is in each cell a thread coiled up in a spiral form. In some of them there is a sort of arrow, with the thread coiled up around the arrow. In others we have a cell of conical shape, and here also a thread coiled up. Upon watching these cells which, from their contents, I could have no doubt were the nettling cells, I have been fortunate enough to see the manner in which these threads are issued like a lasso. I have no doubt that it is with this apparatus that they sting, though I cannot say what is the action produced upon the tissues of other animals to cause the painful sensations they produce, as all this apparatus is too minute to be investigated in any other way than through high powers of the microscope, with transmitted light, but the chemical operation of the fluid to produce such a sensation upon the skin cannot be discovered in this way. The quickness with which these animals kill others which come in contact with them, leaves no doubt that these little microscopic cells, with these threads, are most powerful weapons, by which they attack and kill their prey almost instantly.

How does this thread, which is so long, uncoil and come out from the cavity of the cell? It is as quick as lightning, and therefore the more difficult to observe, as the whole thread, which is 12 or 16 times longer than the longitudinal diameter of the original cell, is thrown out in almost an instant. It is here that I reach the extreme limits of the working power of our microscopes. When the threads are out of the capsule, and entirely protruded, we see upon them, near the empty cell, some indications of curious external appendages, which have been noticed before. It has been said, also, that the thread is twisted; but there is no such thing; no such thing at all. Not one of the threads of the nettling apparatus of the Polypi is twisted. But their external surface when out, appears as if furnished with a spiral of minute hairs, which extend through the whole length of the thread.

I have actually succeeded in tracing this spiral for the whole length of the minute thread, and not simply upon their swollen base near the bulb. We have then a spiral line winding around the whole length of the nettling lassos, and upon this spiral what seems stiff hairs. But this thread is not solid; it is a hollow tube, a direct continuation of the walls of the cell to which it is attached. I have seen how this tube, which is first coiled up in the cell, is inverted like the finger of a glove turned inside out. In observing the cells, three times in succession, I saw the thread thrust out, at first appearing to turn with great rapidity upon itself within, and then, after a part had been pressed out, the extremity of the thread within came in sight and could be traced as it escaped through the whole length of the part already drawn out, until the whole was extended and the point actually projected outwards; so that this fine thread is in fact a tube, and is finally turned inside out to the very extremity of the thread. Now conceive what extraordinary structure this apparatus must have to allow of such a motion! Our microscopes now do not reach at all the limits which they should reach in order to enable us to trace the structure by which such phenomena are produced. I only describe appearances now; but it is evident that an apparatus subservient to such a purpose, and acting with such rapidity, cannot but be highly complicated in its structure, although that structure is so minute as to escape our eye, even when armed with the most powerful magnifying apparatus of the age.

Now having described the peculiar form of the nettling cells of this coral-building animal, let me say what I have further seen about it. The festooned head of a tentacle which is hemispherical, may, from its termination, issue thousands of these lassos at once, so that the summit of the tentacle is then like a formidable wall of peaks standing out in all directions, and between them all are then vibrating cells with their myriads of cilia maintaining currents of water, the threads standing out from their empty capsules. I cannot give a description vivid enough to convey to your minds the idea of such an apparatus as this presents when sent forth against its prey. But you will imagine how formidable must be such an apparatus, when these animals can reach out so far beyond their apparent surface, and stretch forth these unseen organs of apprehension. These animals are, you will at once perceive, to the small living beings around them, their most dangerous enemies, since the radius of their reach is so far beyond their apparent surface, owing to the length of these lassos.

Having now ascertained that these lassos are thrown out by the entire cell, and by the inversion of the thread itself, I would make some further comparison of this apparatus with other cells. It is plain that the lasso-cells themselves are only a modification of cells proper. They arise as cells; among those completely formed are always some not yet fully developed, which are simple bags, incapable of unfolding, and which resemble the cells around so much that you have all transitions from common cells to vibrating cells, and from the vibrating cells to the cells with lassos, of the most complicated kinds. Therefore we must consider these lassos as cells; and the moment we arrive at this conclusion, we have implicitly shown that single cells may be highly complicated organisms, — cells which are, under ordinary circumstances, the simplest elements of organic structure! We have, indeed, now in this case, isolated cells, which are generally the simplest elements of organic structure, presenting a complication of structure not inferior to that of many animals which are considered as highly organized.

Now the vibrating cells, with their cilia, have been long known; but how the cilia are formed, had not been ascertained. I may state, that I have seen in embryos of Crepidula the vibrating cells forming with their cilia still within; the cilia were not additional productions formed upon the cells externally, but simply the margin of the external surface of the cells themselves, turned inside out, like inverted sacks. But it is not only in the position described, where these lassos and these various appendages occur, but upon the whole surface of the little Polypi we find similar nettling organs. They occur upon the inner surface of the wall of the mouth, also scattered upon the whole outer surface of the wall of the body; they occur finally upon those cords which have long been known to exist upon the inner margin of the partitions, the special functions of which we have, however, not yet fully ascertained. These cords, indeed, consist entirely of accumulations of cells, which are covered with lassos of the same diversity as those of the tentacles. Therefore I have no doubt, at present, that the nettling apparatus, these so-called spermatic cords, the spermatic cells themselves, and vibrating cells upon the surface, are only various modifications of cells, showing how highly organized, how highly complicated this simple element of organic structure may be.

It is perhaps a matter of surprise that the coral animal should have been found in this latitude. They teem in the warm latitude; but there are very few species in the more temperate regions, and but for

the opportunity afforded by the Coast Survey, the existence of these animals could not have been suspected on these shores. For many years, however, dead fragments had been found along the shores; but whether they lived there naturally or not, had not been ascertained, as far as I know. If I did not fear to trespass longer upon your time, I would add a few remarks upon the importance, for the progress of zoölogy, of the facilities afforded by the liberal officers of the Coast Survey. Indeed, a few days spent on board of their vessels have always afforded me more information than years spent in other places.

ON MOSASAURUS AND OTHER ALLIED GENERA IN THE UNITED STATES. BY DR. R. W. GIBBES, of South Carolina.

Dr. G. gave the history of all remains of Mosasaurus in the United States, and described three new species.

He stated that Prof. Agassiz's investigations of the relics from New Jersey had settled that there was but one species there. This has been called by Bronn, *Mosasaurus Dekayi*, and is the same which Dr. Morton called provisionally *M. occidentalis*.

The next species was found on the upper Missouri, and carried to Europe by Prince Maximilian Neu-Wied, who was travelling in the United States. It is in the Museum of Bonn, and has been described by Goldfuss as *M. Maximiliani*.

Dr. G. described a small species from Alabama, which he calls *M. minor;* another from South Carolina, which he names *M. caroliniensis;* and a third from Georgia, which he calls after the discoverer, *M. Couperi*.

He then described three genera of Mosasauroid fossils from Alabama and South Carolina, which he severally entitles, *Holcodus columbiensis*, *Conosurus Bowmani*, *Amphorosteus Brambyi*.

Adjourned.

E. N. HORSFORD, *Secretary*.

Second Day, August 15, 1849.

THE meeting being called to order by the President, the proceedings of yesterday were read by the Secretary, Prof. HORSFORD.

Letters of invitation to the Association to visit Collections having been received from the Natural History Society of Boston, the Essex

Natural History Society of Salem, and Dr. John C. Warren, the following Resolution was offered by President EDWARD HITCHCOCK, in behalf of the Standing Committee:

Resolved, That the thanks of the Association be presented to the Natural History Society of Boston, to the Essex Natural History Society in Salem, and to Dr. JOHN C. WARREN, for their invitation to examine their respective collections; and that the members of the Association will gladly avail themselves of these kind invitations, as far as their time and opportunities will permit.

An invitation was received from President EDWARD EVERETT and Lady, to the members of the Association and their ladies, to meet at his house at half past seven o'clock this evening.

Before dividing into sections, the Association listened to the reading of the following paper —

UPON THE PRIME MERIDIAN. BY LIEUT. CHARLES H. DAVIS.

The question is, whether, having a National Observatory, and being about to publish an American Nautical Almanac, we shall still continue to count our longitude from the meridian of Greenwich, or whether it is preferable for convenience, for accuracy, or for other reasons, to establish a new Prime Meridian on this continent.

I will endeavor to treat this question fully, and to present all the practical and scientific views in relation to it of which I am possessed.

It would undoubtedly be for the advantage and convenience of all civilized nations if a general meridian were adopted by common consent; if all longitudes were counted in the same manner and from a single origin. The man of business, the general student, and, above all, the navigator, would profit by this rule; and the man of science would also find it beneficial in removing the necessity for those allowances and calculations occasioned by the variety of meridians, and with them, a constant source of error.

The Congress of the United States, in a report of one of their committees upon a proposition to make the Capitol the first meridian, evinced at an early period an enlightened apprehension of the benefits that would result from the establishment of a general meridian. (*Report of the Committee on Lambert's Memorial*, Jan. 25, 1810.)

But it also was fully aware how little probable such an event was then, and there are obstacles in the way of its occurrence now, which render it distant and doubtful. It is not to be denied that our own situation is, in some respects, unfavorable for originating successfully such a project. If, nevertheless, you should agree with me, in think-

ing that the opinions which have been entertained for a long time by scientific and practical men here and elsewhere, should be again consulted, I shall be most happy, under your instructions, to communicate on this subject with European Astronomers.

In the mean time we are called to decide upon a meridian for present use. This decision is the basis in my work. Hitherto we have used the English meridian of Greenwich; all our geographical positions and territorial limits are fixed according to that, our astronomical calculations are based upon it, our nautical charts and books of navigation are adapted to it, and our chronometers are set to its time. It has been so much our general practice to count from this meridian, that it constitutes a part of our familiar thought and knowledge.

On this account, and especially with reference to the convenience of our wide spread and growing commerce, a change of the old meridian, if necessary, should be reconciled, as far as practicable, to the wants and habits of the country.

The scientific importance of assuming, at present, an American meridian is undoubted. So long as we depend upon that from which we are separated by an ocean, our absolute longitudes remain indeterminate. Such are the difficulties attending the astronomical determination of this element, that the greatest accuracy attainable is only an approximation to the truth; varying, as observations or computations are multiplied, or as new and better methods and values are introduced. There is no place on our coast, the longitude of which from Greenwich is so well ascertained as Boston. The observations and computations made for this purpose by the late Dr. Bowditch, and communicated to the American Academy, bear the marks of his genius and labor. The means of determination have since been greatly multiplied — solar eclipses, occultations, and moon culminations, have been collected in great numbers. The transportation of numerous chronometers between England and Boston has afforded the materials of further improvement. Commodore (now Admiral) Owen, whilst engaged in the survey of the Bay of Fundy, carried his chronometers to Boston, adopting that as his American first meridian, because it was the best determined. Mr. Bond, the Director of the Observatory at Cambridge, has been, for several years, employed in the service of the Government, in accumulating all the means of perfecting the longitude of Boston. Yet I am informed, that there still exists an uncertainty in this longitude, notwithstanding all the labor and care bestowed upon it, to the amount of, perhaps, two

seconds of time. It is, also, a pregnant fact, worth mentioning, that the relative longitudes, even of the Greenwich and Paris Observatories, have been recently changed.

But the uncertainties arising from the intrinsic difficulty of making absolute determinations of longitude increase as the place is more remote, and therefore less known or cared for. The assumption of a new origin of longitude situated in this country, will, to a considerable extent, remove these uncertainties, and save us from those fluctuations in our geographical positions to which we are now subject. In the magnetic telegraph, we have a means of determining differences of meridians, which belongs to the highest order of accuracy. It can be applied at once wherever the wires now run. An American prime meridian being adopted, this should be done as soon as possible. As the use of the telegraph is extended, the Interior, throughout its whole space, would be connected in this manner with the stations of the Coast Survey and the National Observatory, and would have the geographical positions of its chief cities and county towns permanently and unalterably fixed, and thus the foundations would be laid of a correct geographical map of the whole country.

In making a change, however, that is so radical with regard to some of our citizens and their pursuits, great consideration is to be had, I think, for their practical wants and conveniences. These last should be no further sacrificed than the first demand of an independent accuracy strictly requires. Our navigators, and seafaring people generally, are chiefly concerned in the result of this change. Speaking a common language with the greatest commercial nation of the world, our own vessels are constantly meeting those of Great Britain on the highway of nations, and are in the habit of comparing with them their longitudes. From this facility and frequency of intercourse they receive and confer great benefit, upon which sometimes the safety of a vessel at sea may depend. They are also accustomed to employ British charts and chronometers; and it is very desirable to secure the greatest possible facility for their use, and comparison for the future, and in every quarter of the world; throughout all parts of which, our communication with British ships, ports, and means of navigation is intimate and habitual.

For these and similar reasons, it will be better if our own meridian is so situated as to admit of an easy interchange with that of Great Britain, particularly at sea, where intercourse is always necessarily brief, and frequently, owing to circumstances of weather, distance, or haste, very difficult.

If the meridian of Washington, which, as the capital of the country, it will first occur to us to select, be adopted, it will prove unsuited to these emergencies. The longitude of Washington is in time about 5h. 8m. and 16s.; or in space 77° 04′ from Greenwich. These are inconvenient sums to add or subtract; their application is not ready and easy. This meridian will also cause that kind of difference in the division of our charts, the face of our chronometers, the reading of our text-books of navigation, &c. which would seriously interfere with the habits of our present, and the wants of our future nautical men. These, as I have said, are considerations worthy of great regard.

The life of commerce subsists by the mutual interchange of relations, not material only, but personal and intellectual also. These relations are, in our case, much more numerous and complex with Great Britain than with any other nation, on account of her large fleets, her distant colonies, and our community of speech. We may omit to provide for the wants and habits growing out of them, but we cannot alter, indeed as a great commercial people we do not desire to alter, the fact of their existence.

To avoid in some measure the difficulties and inconveniences already stated, and to satisfy, as far as possible, the demands of daily practice, I propose to establish an arbitrary meridian at the city of New Orleans, which will be exactly six hours in time and ninety degrees in space from the meridian of Greenwich. These round numbers are easy in their use and application. They are taken from or added to the headings of charts, the readings of chronometers, or the values in the astronomical ephemeris, without delay, and with little danger of mistake.

They are also convenient for the interchange of longitudes at sea. This meridian cuts the great valley of the West, and approaches to the central line of our territory on this side of the Rocky Mountains. It passes nearly through the centre of the great eastern slope of the continent, and enters the city of New Orleans, the mart and outlet of its products and trade. I propose to call it the meridian of New Orleans, in which city a spot is to be found having this suitable difference of meridian of six hours, or one quarter of the circumference, from our present standard meridian of Greenwich.

This choice has other recommendations, which give it the preference over Washington. The meridian of the latter cuts the seacoast between Cape Fear and Cape Lookout. Our coasting vessels and

domestic packets, therefore, in going and returning between our northern and southern ports would, if it were taken, be subject to the inconvenience of a change of name in the longitude, which, as is well known to navigators, always involves a liability to error.

The meridian of New Orleans, on the other hand, is so far west, that all the longitudes on the Atlantic coast remain on the same side. It is only on the coasts of Louisiana, west of New Orleans and of Texas that they change. This change takes place where the river Mississippi empties its turbid waters into the Gulf, where nature has marked the line by an altered condition of the water, such as may be observed by the careful seaman at some distance from the land.

Another practical recommendation to this choice is this: If New Orleans be adopted, then between the American and English meridians, the degrees and minutes on the chart will be the complements of each other. To the westward of the American meridian, up to 180°, the minutes will be the same, the degrees being less by 90°. To the eastward of the English meridian there is the same advantage; the number of degrees on our part being greater by ninety, up to 180° of the English longitude. Between the inferior meridians of the two nations for the space of 90°, the sum of the American and English longitudes will be equal to 270°, but they will be of different names.

These normal differences are easily remembered, and compare favorably with the confusion that will follow, if the modes of reckoning longitudes by the two nations differed by so unmanageable a quantity as 77° 04′. The time is not distant when we shall have published, under the authority of the government, perfect charts of our harbors and external seacoast. I trust also that the day is not far distant, when foreign charts (improved by surveys made by our own officers) will be issued from the bureau of Hydrography of the Navy Department, for the benefit of the commercial marine of our own and other nations. These charts should be rendered as serviceable and available as possible to the whole maritime world; and this end will be attained in the manner pointed out above. What is for our own advantage will prove beneficial to others.

It may be proper to observe here, that, although I speak only of the meridian of New Orleans as arbitrary, yet all prime meridians are essentially arbitrary. They have been selected always with a sole reference to the national convenience. Some nations, as the French, Portuguese, and Dutch, have placed their prime meridians

out of their own country, but I need not consume space by entering into historical details that are easily obtained. (See *Enc. as Perthensis and Britt.*, Art. *Geo.* — *Good's Pantalogia*, and *London Ency.*, Art. *Meridian.* — *Delambre's Hist. of Astronomy.* — *Mackay on Long.*, &c.) The early conduct of the French, however, in this matter, deserves to be mentioned because it contains some instructions for ourselves. By a royal ordinance of Louis XIII. the island of Ferro was established as the French first meridian, and Paris was assumed to be twenty degrees to the eastward of it. It appears from the memoirs of the Royal Academy of Sciences of 1742 and 1746, that attempts were subsequently made to fix with accuracy the exact distance of Paris from the island, notwithstanding that no precise spot on it had been designated as the origin of longitudes, and that there was a prevailing ignorance as to the topography and shape of the island. The determinations by different persons of course varied, and this caused those uncertainties and fluctuations in the French longitudes, which led to the final abandonment of the assumed meridian. We may profit by this example.

Having decided to take for our prime meridian that great circle of the earth which is 90 degrees or 6 hours from Greenwich, we are to keep to it wherever it may fall. By means of the Magnetic Telegraph, the distance of this circle from the meridian of the National Observatory can be determined with sufficient accuracy, and, being once determined, it is to be regarded as fixed and permanent. If it should be found necessary to make any change hereafter, that change will be applied to the imaginary meridian, and not to the meridional differences of other places from Washington, which are to remain always the same.

The Washington Observatory is thus made the virtual standard according to which all values are assigned, and to which all meridional differences are referred; and from which, also, all *absolute* longitudes are computed.

Its own distance from the six hour circle being once ascertained by magnetic communication, it will be, in effect, for this country, the true origin of longitudes.

It will not be practically indispensable to distinguish by any visible, real mark the meridian of New Orleans, so far as the National Observatory is concerned; for the latter, its distance in time from the arbitrary meridian being once assumed, becomes the effectual, established zero; but this mark will be useful for reference in the adjacent

country, saving labor and time in fixing longitudes in its vicinity, and its foundation appears to be peculiarly proper as a national monument. The cost of such a mark will be but trifling.

Thus the new meridian will be, what its name implies, strictly arbitrary. It may be thought that there are reasons of a scientific character why the National Observatory at Washington should be selected as the nominal origin of longitude, on this continent. Such is not the case. Our National Observatory at Washington must have existed a half a century before it will be able to furnish independent observations sufficient for the determination of a correct theory of the moon or primary planets. But these theories are already calculated from the observations (begun long since and uninterruptedly continued) at the old, established observatories of Europe. In preparing new tables, I shall avail myself of the Washington observations to the utmost extent of their utility.

I propose, also, to give, in the astronomical ephemeris, the times of transit, and the corresponding places of the planets, and principal fixed stars, over the meridian of Washington.

Hitherto I have treated this question without an express reference to merely national views and feelings. So far as the subject is merely scientific, they do not enter; so far as it is practical, they are of paramount importance.

But I am very far from being seduced into a forgetfulness of national sentiments, by the silly pretence that there are or can be duties to science or to humanity, which are at variance with those to country; a notion, wherever it is held, that implies not only a want of patriotism, but of true humanity also. "Science knows no distinctions of country" — in its claims to support and in its exemption from hostilities — in its spirit and in its communions — and in its highest aim, which is to study the laws of nature, and endeavor to make the knowledge of those laws useful to mankind.

But science, like all objects of human interest and pursuit, is compelled to recognize the distinctions of country in the duties it imposes, in the means of its progress, and in some measure in its associations and the limits of its operations. It prospers and is fostered by those affections which divide us into distinct families and nations, at the same time that they preserve our relation to the whole race.

Being designed to act within a limited sphere of usefulness, we are happily supplied with a motive to every duty in a corresponding affection, which, if rightly elevated and directed, renders the performance of that duty easy and agreeable.

Feeling assured that it is by laboring in the sphere assigned us that we are most likely to accomplish something that may be beneficial to mankind, and that by making ourselves good citizens of that State to which our efforts are unavoidably confined, we may best hope to prove useful citizens of the great republic of letters and science constituted by the union of all cultivated people, I indulge a sentiment of American pride and gratification, that another step has been taken by the government towards the promotion of science, by the foundation of an American Nautical Almanac.

Prof. BACHE moved that it be referred to a committee of mathematical gentlemen from various parts of the Union, to be appointed by the President.

The Secretary, in behalf of the Standing Committee, reported the following Resolution, which was adopted :

Resolved, That the Association be divided into two Sections ; one to embrace General Physics, Mathematics, Chemistry, Civil Engineering, and the applied sciences generally ; the other to include Natural History, Geology, Physiology, and Medicine.

The order of proceedings having been announced by the Secretary, the meeting adjourned to meet in sections.

E. N. HORSFORD, *Sec'y.*

SECTION OF NATURAL HISTORY, GEOLOGY, &c.

THIS Section was called to order by Prof. AGASSIZ, and Prof. HENRY D. RODGERS was appointed to the Chair for the day, the Section having decided to choose its Chairman each day. Prof. C. B. ADAMS was elected Secretary.

The papers before the section were then read, as follows :

THE ZOÖLOGICAL CHARACTER OF YOUNG MAMMALIA. BY PROF. LOUIS AGASSIZ.

Zoölogists, in their investigations, have constantly neglected one side of their subject, which, when properly considered, will throw a great amount of new light on their investigations. Studying animals, in general, it has been the habit to investigate them in their full-grown condition, and scarcely ever to look back for their characters in earlier periods of life. It is only among the insects that naturalists have collected facts as to their metamorphoses ; owing, perhaps, in the

beginning, more to a desire of securing particular specimens in a proper condition for observation, than to investigate their transformation; until the interest awakened by the facts which came to notice in this way called for some precise investigations upon the subject. And, since that time, the history of the transformations of insects has been studied very extensively. In other branches of Natural History, however, the young animals have been almost entirely neglected. We scarcely ever find, in a book on Natural History, a hint as to the difference which exists in the young and old. Perhaps in birds the color of the young may be noticed; and it is generally known that the young resemble the female more than the male; but as to precise investigation of the subject, we are deficient. But if the early stages of life have been neglected, there is one period in the history of animals which has been thoroughly investigated for the last twenty-five years, the changes which take place within the egg itself, and which give rise to the new individual, have been thoroughly examined, and constitute a science by itself, — Embryology; but, after the formation of the new being, the changes in its form which it passes through, up to its full-grown condition, that point has been neglected; and that is the point to which it is my purpose to call your attention for a few moments.

It was my object to investigate this subject, on account of my having been struck with the deficiency there is on this point in our works. And if I may be allowed to make a digression on this occasion, let me say that the best rule for students to pursue in investigating, is, not to look for the departments of science where they may find the most information to guide them in their studies, but to look for a department upon which we have ascertained but little or nothing. Where others have not reaped, there is a rich field for examination, and the less there has been done, the richer the harvest to be expected.

Now, proceeding in this investigation, I found that the young animals, in almost all classes, differ widely from what they are in their full-grown condition. I will state at once a few of the results. For instance, a young bat, or a young bird, or a young snake, at a certain period of their growth within the egg, resemble each other so much that I defy the most able zoölogist of our day to distinguish between a bat and a snake, or to distinguish between a robin and a bat, or to distinguish between a robin and a snake. There is something of high significance in the fact that there is something common to all these animals. There is a thought behind these material phenomena which shows that they are all combined under one rule, and that they

only come, under different laws of development, to assume, finally, different shapes, according to the object for which they were introduced. The specimens which I here submit to your inspection may serve as evidence of the correctness of what I have just been saying. Here are a young bat, taken from the mother long before it could have been born, a young robin from the egg, upon which the mother sat for a week, and a very young squirrel; I have not been able to procure a snake at that age, within the last few days.

It will be obvious, from a comparison of these specimens, that at a certain time in the growth of these animals, if we were to be guided in their classification by their external appearance and by their external characters, we should put them all together into one class; and that it is only after they have assumed their final characters that we find in their structure, their mode of living, and their habits, the means of distinguishing them, and grouping them according to more minute peculiarities, and of recognizing, finally, those general characters upon which our classification rests.

Is there, however, any use in studying these early stages? it may be asked — as we value too often our investigations according to the benefit we may derive from them. I say, yes, and a very important use. The investigation of these successive changes gives a natural scale in which we may place, according to their peculiarities, any type we find which has not passed through all the changes which others undergo. If we find a period when the young mammalia, the bat and the squirrel, resemble in form the bird and the snake, and if we find them, full grown, so vastly different from each other, as they pass through successive changes, gradually becoming more different, then we have here a gradation of transformations, to which we are enabled always to refer any type which may remain at a given step, and assign to it its proper position in the gradation.

My object at present is only to allude to these common characters of mammalia, and to show how they may be used to improve and correct our classifications of these animals. Let me first state what are some of these remarkable peculiarities in mammalia. I have named, for instance, the squirrel and the bat. When full grown, or far advanced in their development, they look very different, especially in the legs; the bat having very long fingers, and a membrane extending to their very extremity. There is indeed in the bat a shoulder, a forearm, a wrist with few bones, a long thumb, besides four fingers, as is well known.

Now, this arm, the bones of which are united by the membrane into a wing, is, in the young animal, simply a fin-like, short appendage. It is nothing but a short fin, in which the indentations of the fingers are scarcely marked. There is in fact a period when the hand itself is only a large flat fin, without any distinction of fingers at all. Now the development of this hand consists, first, in a division into fingers; then the fingers are elongated in the same proportion as the arm itself; and finally that fin is transformed into a wing. The hind extremity, at the beginning, consists also of such a little paddle, first without division, and then with a slight indication of fingers, and finally a complete division into five fingers, with their regular joints. So different as the hind foot is from the fore foot in the full grown condition, you see how perfectly identical they are in their embryonic condition. We may derive from this fact an important lesson for our classification; that notwithstanding the great difference there is between the bats and other mammalia in their organs of locomotion, we might be induced from their peculiarities to bring them together with other animals from which they are now far removed, if we were to find that they agree in other respects, — and, indeed, this is the case, for there are no animals more closely allied in their structure than the moles and other insectivorous mammalia and the bats, so that in the natural classification we must bring them together, disregarding the extraordinary difference in their organs of locomotion.

The fact that the fingers in the bat's wing remain united, far from assigning these animals so high a position in the natural scale of mammalia, would, in my opinion, be rather a reason for considering them as lower in their natural position; but I will not take up your time with more details.

The main object I have in view is plain from the few statements which I have made. In all the mammalia, of which I have observed the young, and I may add that I have already found the same uniform structure of the organs of locomotion in many of them, there is a period of life, in which, whatever may be the final form of their organs of locomotion, whatever may be the final difference between the anterior and posterior extremities, vertebrated animals have uniform legs in the shape of little paddles or fins. This is the case with lizards as well as birds. A robin's wing and a robin's leg, which are so different from a bat's wing and a bat's leg, do not essentially differ when young from the leg and arm of a bat. Again, wherever

we observe combined fingers preserving this condition, we have a decided indication that such animals rank lower in the group to which they belong, than those with distinct fingers. This is all-important, as we are enabled at once to group animals which are otherwise allied, in a natural series, as soon as we know whether they have combined or divided fingers. Even the degree of division to which the legs rise in their development is a safe guide in our classification. Look, for instance, at the legs of dogs and cats, in which the fingers are completely separated, and so elongated that the animals walk naturally upon tiptoe, and compare them with those of other Carnivora, bears, for instance, which walk upon the whole sole of the foot, and again with those of seals or bats, which remain united, and constitute either fins or wings. There can scarcely be any doubt that Carnivora should be arranged in such a series: Pennipedia, Chiroptera, Insectivora, Plantigrada, Digitigrada.

We have sufficient other reasons to be satisfied that the order of arrangement which I assign them, according to the development of the fingers, is justified by the state of development of the other organs of the mammalia, and especially of their higher organs and intellectual faculties and instincts. I may add that mankind is not excluded from this connection, but that, in common with other vertebrata, we are all at one stage of existence provided with paddles or fins, which afterwards are developed into legs and arms. Slight indications of this primitive condition are even observed in the black race throughout life, in the closer connection of their fingers.

Dr. HARE made a few remarks upon the mystery of the law of development, which seemed to be entirely distinct from any chemical power, or from any general law. There seemed to be special laws for every form.

THE VEGETABLE CHARACTER OF XANTHIDIUM. BY PROF. LOUIS AGASSIZ.

IT is a sad duty to be obliged to object to views and statements of friends; but those who seek for truth above all, take objections with as much pleasure as additions to their knowledge; because objections, when founded in truth, are really additional knowledge acquired; and I know too well the spirit of EHRENBERG to doubt that, if he were present, he would feel no dissatisfaction at the statements which I am about to make, though it touches upon one of the most sensitive

chords with him. He was the one who combined with animals, and introduced among the animalculæ many living beings which, up to the time of his investigations, had been considered as doubtful, forming, as it was supposed, a kind of intermediate link between animals and vegetables. These Infusoria, as they are generally called — little beings found everywhere in the stagnant waters — were considered as somewhat indefinite in their real organization, until Ehrenberg, basing his conclusions upon the most extensive investigations which had ever been made upon the subject, upon extraordinary discoveries in their structure, asserted that they were all decidedly of animal character, and introduced among them many which several authors referred with confidence rather to the plants than to animals. Now that of which I am to speak is one of this last class. Xanthidium, however, was established as a genus by Ehrenberg.

It is a microscopic body, found floating upon fresh waters, consisting of a central sphere with radiating hooks, the points of which are bent like an anchor. These hooks are in various numbers around the central discs. These bodies allow, perhaps, some doubt as to their nature; but Ehrenberg asserted that he had discovered within their body, granules which he considered as eggs; and he described several species. He ascertained even the existence of these bodies among fossils, many of which are found in tripoli and various other tertiary deposites. Some of them are even found in opal, well preserved, silicified; and they have been found also, by Dr. Mantel, in the chalk of England.

On the consideration of their affinities to Micrasterias and other seeming fresh-water animals, Ehrenberg referred these curious beings, without doubt, to the family of Bacillariæ, and considered them as animals. I have been so successful as to secure living specimens of Xanthidium from the fresh waters about Boston, which settle the question, and show that Ehrenberg has been mistaken, and that Xanthidium belong to the vegetable kingdom, and that what he took for an animal is only a Sporangium, the seed capsule of this plant. In fresh water here about Cambridge, in Cochituate Lake, and in various other fresh waters in the vicinity of Boston and Dedham, for instance, they have been found, and I have received from several friends specimens of this vegetation, which consists of larger or smaller masses of gelatinous matter attached to branches hanging in the water, which grow to large bulky bodies, and assume various forms, and upon which are found in time, innumerable Sporangia, discoidy bodies, with hooks at the margin; these Sporangia when first falling from

the plant are enclosed in a certain amount of gelatinous matter; and no doubt what Ehrenberg has mistaken for eggs, are nothing but the sporæ themselves, existing within these bags.

The structure of these Sporangia is interesting in itself. I will make some mention of what I have observed. The whole capsule consists of most beautiful hexagonal cells. These form a pavement all over the surface, while the hooks are covered with elongated cells, and the whole enclosed in jelly; those cells all over the body, give the whole the most elegant appearance, when seen under a highly magnifying power. I have asked myself what might be the use and object of this gelatinous mass around the Sporangia, and all the hooks which are attached to the cavity of the Sporangium itself. Ehrenberg did not notice this jelly, and indeed it is often wanting. It is entirely wanting in specimens which have long been kept in water. Is it not very likely that the gelatinous cover facilitates the attachment of these bodies to branches or leaves, or any body floating in the water, and that they remain still more entangled in these branches, by the hook, as soon as this gelatinous cover is dissolved in the water, and that they are thus kept in the places where they are to grow?

Prof. Gray remarked that a similar gelatinous investment or nidus is of very common occurrence in the lower Algæ and in Diatomaceæ. He remarked that the Xanthidium, and the drawings now exhibited by Prof. Agassiz looked very unlike the fructification of any proper Algæ, but they might be compassed with some forms of Diatomaceæ, an ambiguous group, which had only recently made good its claim to admission into the vegetable kingdom, and was now sometimes appended to the Algæ, and sometimes taken as a separate order. He agreed with Prof. A. that Xanthidium, whatever its particular nature might prove to be, was of vegetable origin, and that, from the drawings exhibited, it was most probably allied to the Sponges.

Mr. Mantell remarked that he had frequently examined these bodies in England, both in a recent and in a fossil state. They had been particularly investigated by Mr. Bowerbank, who had, he believed, arrived at the conclusion that they were the gemmules of sponges.

Prof. Agassiz added, that when he referred Xanthidium to Algæ, he did not mean Algæ proper, but the large group of Hydrophytes, including Algæ and Sponges.

On Valerianate of Morphia: A New Medicine. By Dr. M. Wyman and Prof. E. N. Horsford.

Prof. Horsford remarked, that at the suggestion of Dr. M. Wyman, he had prepared, several months since, a quantity of Valerianate of Morphia, a new salt. The Valerianic acid was made by the oxidation of Fusil oil, — one of the incidental products of fermentation in the manufacture of alcohol. This oxidation was effected by means of bichromate of Potassa and Sulphuric acid. The acid distilled from the solution was converted into Valerianate of Baryta, and this salt, by double decomposition with Sulphate of Morphia, was resolved into Sulphate of Baryta, which fell as an insoluble powder, and Valerianate of Morphia, which, after filtration and concentration, crystallized in beautiful forms of great transparency.

Several analyses revealed the fact, that there were two salts, one of which parted readily with its water, efflorescing upon exposure to the air, and the other of which was permanent.

Dr. Wyman, who had carefully observed the effect of this medicine, has furnished the following communication: —

It is well known to the physician that Opium, besides procuring sleep, allaying or entirely removing pain, and suspending the mucous secretions, also produces other and undesirable effects, which materially diminish its usefulness. Various attempts have been made to prevent these effects, at first by using different solvents of the drug, and, afterwards, by separating the morphia from the other substances with which it is combined; usually, on account of the greater solubility of these salts, in the form of a sulphate, a muriate, or an acetate. Although the objectionable properties of opium are diminished with most persons when taken in these forms, still there are some who suffer as much from the one as the other. Neither is it known that the acids in the salts just mentioned have any medicinal influence in themselves when so combined, or that they materially change the action of the morphia; although it is so well known that the therapeutic effects of Opium are very materially changed by being mixed or combined with other drugs. With the hope that the Valerianate of Morphia prepared by Prof. Horsford might possess properties different from, and more valuable than the other salts of Morphia, it has been submitted to trial.

In small doses it is found to produce more quiet sleep, and to be equally efficacious in removing pain with its equivalent in crude Opium, or the Salts of Morphia. In a case of violent nervous excite-

ment it acted most favorably, producing quiet and sleep after other preparations had failed. It has been given in a few cases in which, from constitutional peculiarity, a feverish state ensues, with watchfulness and starting instead of sleep, or quiet reverie. In these the sleep was not continuous, but the intervals of wakefulness were shorter, and the general frame of mind more calm. The subsequent effects, headache, nausea, and vomiting, were decidedly less than after an equivalent of the other preparations. Itching of the skin and nose are produced by the Valerianate in those individuals who experience it after taking Morphia in other forms. One individual who always experiences an attack remembling colic after taking Opium or Morphia in any form, suffered from this preparation also.

In full doses, also, the subsequent effects are less. In dysentery this has been observed in a marked degree. The doses were from one-third to half a grain, repeated from eight to ten times in twenty-four hours. The secretions were lessened, the evacuations controlled, and the pain removed, with less headache, nausea, and vomiting. These are usually produced by an equivalent of the other salts of Morphia. These results obtained from a limited experience with the Valerianate prepared at the Cambridge Laboratory, warrant a more extended trial by physicians, that its value as a medicine may be correctly ascertained.

The dose is about one-fourth that of crude Opium ; it is most conveniently given in the form of a pill.

[This medicine, Prof. H. had understood, was already manufactured in Philadelphia, and would doubtless be soon for sale by apothecaries generally.]

On the Mastodon angustidens. By Dr. John C. Warren.

Dr. J. C. Warren made remarks on the existence of osseous relics of *Mastodon angustidens* in the United States, under the following heads : —

First. Distinction of the Mastodon race into two species, viz. : Mastodon giganteus and Mastodon angustidens ; the former being found in North America, and the latter in Europe, Australia, and South America.

Second. History of the discovery of a tooth, supposed to be of the Mastodon longirostris of Kaup.

Third. Examination of this tooth and comparison of it with that of M. giganteus of the United States, and of M. angustidens of

Europe, and proof adduced to show that it belongs to the latter species.

Fourth. That it also nearly resembles the Mastodon teeth of South America, and probably belongs to the same species.

Fifth. Minute detail of facts, intended to show that it was actually dug up in the State of Maryland.

Sixth. That it is a Miocene fossil, like many similar teeth in Europe, and has, of course, a higher antiquity than relics of M. giganteus.

Seventh. Reasons why other relics of the same species have not been discovered, and why it is probable they will be hereafter.

Dr. Warren further remarked, that the subject was one which he considered fairly open to investigation, both in relation to the number of species into which the Mastodon Family should be divided, and as to the probable existence of remains of M. angustidens in this country; and that he had brought the subject before the Association in the hope of being aided by their science in the settlement of these two questions.

Prof. Rodgers expressed some doubt with regard to the existence of the Mastodon angustidens in this country, as but a single tooth of a single being had yet been discovered, though the Miocene deposits of the seaboard country of the United States had been most thoroughly probed; while the fact of this tooth's having twice crossed the Atlantic, afforded room for suspecting that this was not the identical tooth discovered, notwithstanding the careful researches of Dr. Warren.

Prof. Agassiz stated that there were sixteen different species of Mastodon described at present, and that he conceived there were good reasons for distinguishing more than two species. They had not, however, been grouped into distinct sections. He would admit Dr. Warren's statements, as showing conclusively that they should be divided into two groups, the broad-toothed and the narrow-toothed; but at the same time, he would insist upon the propriety of admitting several species in each of these groups.

Dr. Warren remarked that there was great diversity of opinion as to the number of Mastodon species which ought to be admitted. A distinction of M. giganteus into more than twenty species had been proposed, and of M. angustidens into a number not exactly defined. He was desirous that the subject should be fully examined, and would be glad to find reasons for admitting a greater number of specific differences than he had yet been able to discover.

Dr. GIBBES stated that there was in the South Carolina College, at Columbia, a fine specimen of a tooth of Mastodon giganteus, which was found in a bed of *Pliocene* marl in Darlington District, S. C. He had also in his possession portions of bones from similar deposits in Sumter District, S. C. Prof. Tuomey's investigations have induced him to refer these deposits to *Pliocene*, which were formerly considered *Miocene.*

ON THE ISOLATION OF VOLCANIC ACTION IN HAWAII, OR VOLCANOES NO SAFETY VALVES. BY JAMES D. DANA.

The observations here presented, were made during the cruise of the Exploring Expedition under Captain Wilkes, and are detailed with many additional facts and a fuller exposition, in my Geological Report. The subject has a wide bearing on geological theory, and is therefore brought forward for the consideration of the Association.

The Island of Hawaii has an area of about 3800 square miles. Its shape is triangular, with a western side 85 geographical miles long, a southeast 65 miles, and a northeast 75 miles. The whole surface of the island pertains to the slopes of three lofty volcanic cones or domes: One, *Mount Loa*, (Mauna Loa,) occupying the southern portion of the island, according to the observations of Captain Wilkes, is 13,760 feet high; another, *Mount Kea*, constituting the northern portion, is 13,950 feet; and *Hualalai*, near the middle of the western side, is about 10,000 feet in altitude. Besides these heights, there is a single independent ridge, called *Kohala*, on the northern side. The great plain, or intervening country between the mountains, is about 5000 feet above the sea.

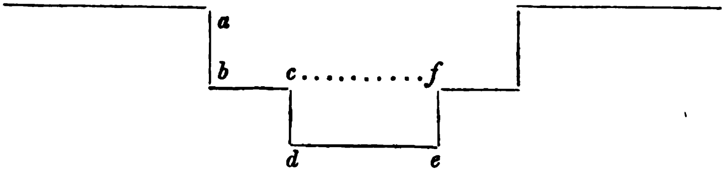
The slopes of Loa and Kea average but 6 to 8 degrees in inclination, and on the southeast and east, the declivities of the former diminish to 1½ degrees. To conceive rightly of the island, the gradual even character of these declivities should be borne in mind. Loa is a gently rising dome with a broad flat summit, and no slender pointed cone, as a volcano is often pictured to the fancy. If we slice off from a 12 inch globe a segment about half a line deep, we have a good model of its general form. Kea has nearly the same slopes, but gradually rises to a pointed summit.

Of the three volcanic mountains of Hawaii, Mount Kea has been long extinct. Hualalai is still smoking, and was last in eruption about the year 1800. Mount Loa, as is well known, is in full and vigorous activity. Other cones are scattered in great numbers over

the island, but they are subordinate to one or the other of these three great volcanoes, and though from 100 to 1000 feet high, they are but unevennesses on their sides or about their bases.

Mount Loa has at its summit a large pit-like crater. It is somewhat elliptical in shape, with its diameters 13,000 and 8,000 feet, and a depth of 784 feet. There are no thin walls around it as about Vesuvius; it is like a vast excavation in the wide summit plane. Through fissures in the bottom of the pit, vapors are constantly rising. But at times the action is intense, and eruptions take place. On the 20th of June, 1832, there was an eruption from the summit, discharging on nearly every side of the dome, and continuing in progress for two or three weeks. In January, 1843, another eruption took place, upon which we have observations of great interest, by Messrs. Coan and Andrews. The lavas broke out at the very summit and flowed down the slopes in two great streams, one westward towards Kona, the other northward to the foot of Mount Kea, thence dividing and continuing part northeastward towards Waimea, and part eastward towards Hilo, this latter line making a total length of 25 or 30 miles, with an average breadth of 1½ miles. For about four weeks the eruption was unabated; and after six weeks, there was vehement action at some points on the line.

Besides the summit crater of Mount Loa, there is also the still larger of Kilauea, (pronounced Kee-low-ay-ah,) situated on the southeastern slopes of Mount Loa, about 4000 feet above the sea. This is another pit-crater, looking like a vast quarry hole in the rock-built structures, and having in a distant view the horizontally stratified sides of a limestone gorge. The gentle slopes of the dome here scarcely vary from a plain. It is an amphitheatre of rock, 7½ miles in circuit, and 3½ miles in its longer diameter, with a depth of 1000 feet — large enough, in fact, to hold 400 such structures as St. Peter's at Rome. Conceive of a pit of such a depth, stretching from the Colleges to the centre of Boston, and embracing a circuit of 7½ miles, and some impression will then be had of the vast extent of this crater. The walls are precipitous bluffs of stratified igneous rock, except on the southeast, where there are steep slopes of decomposed lava. 650 feet down, the whole interior is skirted by a gallery or plain called the black ledge, (*b c*) which is one to three thousand feet wide; from the edge of the gallery there is a second precipice (*c d*) of nearly 350 feet, beneath which lies the bottom plain, (*d e*) — a broad area of naked gray and black rock and boiling pools of lava.



Transverse Section of Crater.

In the ordinary state of the volcano, all seems remarkably quiet. The bottom plain is 2¼ miles long, and averages ¾ of a mile in width. When on the spot, six months after the eruption in 1840, there were wreaths of vapor rising from a few parts of its wide surface, and in three spots the red-hot lavas were in constant ebullition. One of these lakes of lava, in the south extremity, measured 1000 by 1500 feet in its diameters, nearly equalling in area the Boston Common. Over its surface, jets were constantly playing, precisely like jets over a boiling caldron of water; yet larger in the more viscid fluid, for they rose to a height of 40 to 60 feet. Standing on the brink of the crater, not a sound was heard from the active fires below; and when on the black ledge, directly over the great lake, there was only the grum muttering of incessant ebullition. Once in a while the lake boils over, and the lavas flow off for one or two miles along the bottom of the crater, spreading a layer of rock, 5 to 25 feet thick.

At other times, Kilauea is in full ignition through the larger part of the whole interior; the caldrons are more numerous and extensive, and there are many spouting cones over the surface, and frequent detonations are heard. Yet it is still characterized by active ebullitions and occasional overflows. Such a scene, covering an area more than seven miles in circuit, must be terrific beyond description, although the "sea" be no sea, and the "waves" but the agitations of a boiling and overflowing fluid.

In all recent eruptions of Kilauea, the lava has broken through fissures in the sides of the mountain below, sometimes appearing at the very brink of the pit, but oftener a few miles distant down the slopes, and thence continuing towards the sea. Thus in 1840, the lavas broke out six miles from Kilauea, and appeared along a track for twenty-five miles, extending to the coast village, Nanawale, issuing from fissures all along the route; and for the last twelve miles forming a continuous tract of lava, averaging one mile and a quarter in width. Previous to the eruption, the lavas stood in Kilauea 350 or 400 feet above their lower level — (at the dotted line cf in the pre-

ceding figure); at the outflow, they sunk at once to the lower level, which has already been pointed out (*d e.*)

Such in brief are the characters of the two great craters of Mount Loa. At the time of the summit eruption in 1843, the inhabitants of the island had no knowledge of the outbreak, until a glare of light became visible after the action had begun; and through the progress of the eruption no sounds were heard below, and no vibrations of the ground perceived. The mountain was rent for at least twenty-five miles, and still not a murmur reached the residents at Hilo, on the eastern shores. In the same manner, at the eruption of Kilauea, in 1840, there were no subterranean thunderings, no shaking of the island to its centre; only slight tremblings were felt in the region of the outbreak. The lavas were discharged through opened fissures, and flowed on with new accessions from other fissures, and finally reached the sea.

With this concise statement of the features and mode of action at Mount Loa, I may now remark intelligibly on the isolation of the lines or conduits of volcanic action.

a. The first fact which I mention is of very common occurrence, and alone might hardly excite inquiry. It is this: the boiling pools in the bottom of Kilauea show no sympathy in their conditions; one may sink 100 feet, while another is overflowing. The smaller pools may boil on at their ordinary level and overflow, when the large lake, 1000 feet in diameter, has sunk 100 or 150 feet below the bottom plain of Kilauea.

b. Again: Although the pit Kilauea is 600 to 1000 feet deep (the depth varying with its different phases,) eruptions, as in 1832, sometimes take place through the very top of its walls, so that the lavas will at times come to the surface at the brink of the pit, and flow back down to the black ledge; and this, while the great pools of lava are open hundreds of feet below, and in constant free ebullition.

c. Again, at the eruption from the summit of Mount Loa, in 1843, when streams of lava flowed out for twenty-five miles in different directions, and the action continued for a month, Kilauea was boiling at its usual rate, without the slightest disturbance or signs of change, or appearance of sympathy. It was visited by Mr. Wilcox, of the Missionary Board, when the eruption was in progress, and he reports the perfect quiet and undisturbed regularity in the condition of that crater.

Is it not a surprising fact that eruptions should take place at an

elevation of 13,760 feet, when on the slopes of the dome, sixteen miles distant, there is an open vent, like Kilauea, more than three miles in length, and 10,000 feet lower in elevation? Why is there no relief here for the vast accumulation of pressure? This pressure when the central conduit is filled to the summit, amounts to 17,200 pounds to the square inch. How is it that the wide, open passage, which Kilauea appears to present, affords no escape for the imprisoned lavas? How is it possible, if the two great conduits, that of the centre of Mount Loa, and that of Kilauea, intercommunicate — how is it possible that the heavy rock fluid stands 10,000 feet higher in one leg of the syphon than in the other? It is certainly difficult to conceive how the ordinary principles of hydrostatics can be so set aside. From the quiet character of the eruptions, it is apparent that there was no paroxysmal elevation of the lavas to the summit; it was a slow and gradual result.

Whatever mode of solving the difficulty be adopted — and I do not propose to enter upon the subject now — one conclusion is evident: that *Volcanoes are no safety-valves of the globe*, although often so called. Assuredly if while a vast gulf is open on the flanks of Mount Loa, lavas still rise, and are poured out at an elevation of 10,000 feet above it, Kilauea is no safety-valve even to the area covered by the single mountain alone. If lavas may be ejected from the very lip of Kilauea, while the pools are still boiling within it several hundred feet below, Kilauea, notwithstanding its extent, the size of its great lakes of lava, and the freedom of the incessant ebullition, is not a safety-valve that can protect its immediate vicinity. How, then, with so limited a protecting influence, can it relieve from danger a *neighboring* island? How can the narrow conduit of a volcano relieve continents from the great earthquakes that sometimes traverse their whole extent?

Volcanoes are in fact indexes of danger; they point out those portions of the globe which are most subject to convulsions. Earthquakes and eruptions are often allied results of the same general cause. As the volcano becomes more active, the earthquakes of the region become more frequent; and the latter cease when quiet follows an eruption. This is true, for the very plain reason that the volcano is the source of danger. When it approaches extinction, the quiet is of longer and longer continuance; and as it dies out, a region once tottering on subterranean fires may finally enjoy the firm stability of lands that have never been favored with such "safety-valves."

Many other considerations bear on this subject, but a discussion of them at this time would lead to a long dissertation on the nature of volcanic action.

Mr. DANA having intimated that he was about to present another paper upon the subject of volcanoes, discussion was postponed until that paper should be received.

Prof. AGASSIZ presented to the Association some specimens of a new species of *Cyanea fulva*, (Jelly fish) still living, and remarked that the nettling apparatus was much easier to examine upon this species of animal than upon the coral.

ON THE FOSSIL REMAINS OF AN ELEPHANT FOUND IN VERMONT. BY LOUIS AGASSIZ.

Prof. AGASSIZ, bringing forward the tooth and tusk, remarked that the Mastodon was not the only big beast about, and proceeded to show that this was not of that species. The teeth did not present the mamillated surface of the Mastodon teeth, but were flat and grooved, as in the living species of the elephant of Asia and Africa. It had been found, a few weeks ago, in the construction of the Rutland and Burlington Railroad, upon the slope of Mount Holly, one of the highest mountains in Vermont, and, it was said, under erratic boulders. It was his intention to examine the very place where they were found, as it would be of great importance to ascertain at what period during the diluvial ages this animal lived. The specimens had been presented to the museum of the Lawrence Scientific School, by Mr. Samuel Henshaw, of Boston. This was the first true elephant found in a fossil state in the Northern American States. It was certainly not the same kind of elephant which had been found in the Kentucky cave. It was a question whether this was identical with the fossil European elephants or not. He deeply regretted that there were no specimens with which he could compare these teeth, but he would venture from recollection to predict that upon direct comparison they would be found to differ from the Europeans, in the same proportion that the Mastodons differed. He thought these grinding teeth had much narrower lamellæ, and that the tusk was much more slender. The curve of the tusk was scarcely greater than in the Asiatic elephant, while the European fossil was much more curved. It would be well to reëxamine all the

species, in order to ascertain whether there was sufficient evidence agreeing to distinguish different species in their geographical distribution. Materials were now abundant, and such an examination had not been attended to since the first collection of loose materials. He considered this a very valuable contribution to the scientific school; but he was sorry to say that it was in itself the museum, which was just beginning to be formed.

Dr. Warren remarked that this discovery formed an epoch in the palæontology of New England. North River seemed to have separated the animals of New England from those of the continent.

Prof. Rogers remarked that he had already, several years since, presented his views to the Association respecting the physical geography of this part of the United States, at the era of the drift. He had showed that New England and New Brunswick constituted an island, detached from the continent, like Great Britain at the present day. From the researches, chiefly of Mather, Emmons, and others, we must now admit that there were two drifts. Up to the time of the first, the Mastodon could not have crossed the straits.

The Section then adjourned.

C. B. ADAMS, *Sec'y.*

Second Day, Aug. 15, 1849.

SECTION OF GENERAL PHYSICS, &c.

Morning Session.

The section of General Physics was called to order by Prof. Henry, the President of the Association. On motion of Lieut. Davis, Prof. W. R. Johnson, of Washington, was called to the chair, and B. A. Gould, Jr., of Cambridge, appointed Secretary.

On motion of Prof. Peirce, the Standing Committee of the Association was requested to arrange the business of this section.

A paper was then read by Rev. Thomas Hill, of Waltham, upon the Natural Classification of Curves, founded upon circular coördinates. Mr. Hill remarked that these coördinates, recently noticed by Rev. W. Whewell, were first given by Prof. Peirce, in the first volume of his Curves and Functions, and still earlier used by him in solving several questions in Gill's Mathematical Miscellany.

They were doubtless worthy of further investigation, and for this reason he would invite the attention of mathematicians to the subject. By circular coördinates he meant the length of the radius of curvature, and the angle which it makes with a fixed direction; that is, a curve is expressed by an equation between the radius of curvature and its direction. The remainder of this paper was occupied with applications of this method to particular curves and general problems.

Prof. PEIRCE followed with a few remarks upon the subject of this paper, and observed that this system of coördinates had enabled him to solve problems which it was impossible to do by the former systems.

Prof. CASWELL, of Brown University, inquired whether this system possessed any peculiar advantages in solving the great problems of nature.

Prof. PEIRCE replied that he thought it especially useful in solving all problems relating to flexible surfaces.

The next paper was read by Prof. PEIRCE, on the Planetary Perturbations.

COMPARISON OF THE RESULTS OBTAINED IN GEODESY BY THE APPLICATION OF THE THEORY OF LEAST SQUARES. BY PROF. A. D. BACHE.

THE most elegant method of reducing the results of a geodetic work is by the application of the theory of least squares, and the finish of the results has been held to justify the great labor of the computations involved. Bessel's volume on the survey of East Prussia is a model of this kind of work. The real applicability of mathematical investigations to physical phenomena is to be tried by the accordance of the several results with observed facts and with each other. Though the mathematical basis of the theory of least squares may be undoubted, yet the application of it in geodesy may be questioned and put to the test of accordance in its several results. A beginning of such an investigation, as afforded by part of the primary triangulation of the Coast Survey, it is my purpose now to communicate.

The first question presented is, whether all the observations of a direction or of an angle shall be allowed the same weight. In some cases the signal observed upon is well defined and appears steady; at others it is ill defined and apparently moving more or less irregularly. If the image of the sun reflected by a heliotrope is used, it

varies greatly in apparent magnitude. In some cases which were computed, in which the observations were divided into two classes, one containing only cases in which the image from the heliotrope was distinct, steady, or moderately so, and the others in which it was large and in motion in different degrees, the weights, which are inversely as the squares of the probable errors, are as two to one, mostly in favor of the first class of observations. Judging, then, by the test of the coincidence with the mean, the first observations should be allowed nearly double the weight of the second.

But there is another way in which their value may be tested, by the accuracy with which the angles derived from them fill the triangles. In four cases which were taken for illustration, the errors in the sum of the three angles of the triangles were: From the means derived from the first kind of observations alone, —2″.04, —0″.74, —1″.23, and —0″.84. From the means derived from the second class of observations alone, —0″.75, —0″.56, —1″.76, —0″.36; the second class of observations actually filling the triangles better than the first.

In the quadrilateral connecting the same four stations of the triangulation at which the observations first referred to were made, the side equations were from the results of the first kind, in logarithms 9.8682650=9.8682652, and from those of the second kind, 9.8682654=9.8682654: the first presenting a contradiction in the seventh place of the logarithm only, and the second an entire agreement.

It appears, then, that the conditions of the figure are at least as well fulfilled by the observations of the second class as by those of the first, and that both should be used with the same weight, as Bessel has already done, in equating the directions. The advantage of using the mean of many observations, under various circumstances likely to eliminate errors of lateral refraction, which were sometimes greatest when the atmosphere was most clear, was pointed out.

The probable error of one measure of an angle with the thirty-inch theodolite of the Coast Survey, was next investigated, and shown to be quite uniformly 1″.3, this including both the error of position and error of printing and reading. The corresponding error with the fifteen inch theodolite used in the Prussian survey, was given by Bessel as 1″.82, giving weights in the proportion of two to one. The telescopes are in length nearly in the proportion of forty-four to nineteen and a half. The result, it was remarked, was so far favora-

ble to the use of the instruments of the greater power, notwithstanding some of their inconveniences.

In deducing from the observations at five stations, the probable error of observation, using all the observations, generally not less than thirty in number, on each of six angles, considering them as belonging to the same series, the probable error was found to be about $\pm$ 0''.22 for an angle. This result, it was remarked, covered or concealed a residual error, which in the method of observing where the position of the theodolite was changed at each observation, was combined into the actual error of pointing and reading. Where several observations are made in one position, these errors are readily separable. This error of *position* was shown from the thirty inch theodolite to be about 0''.17, as affecting the mean value of a measured angle. The probable error of an angle deduced from the mean of the several positions and observations, is $\pm$ 0''.27. These numbers would, no doubt, be subject to modification by increasing the number of cases examined, but were not materially in error. They give for the probable error in the sum of three angles of a triangle $\pm$ 0''.47. This result of the theory may now be compared with that furnished by the corrections shown to be required in the triangles composing the primary series. Nineteen of these give for the mean error in one triangle, disregarding signs, 0''.83, or nearly twice the probable errors deduced from the discrepancy of observations from the mean. The results again disagree, displaying a *residual error* equal to one and one-fourth times the error determined by the first mode of investigation.

The next comparison made was that of the errors in nineteen triangles, as furnished by the method of least squares, and as derived from the measured angles in which a simple mean was taken with some not very important corrections. The sum of the corrections in the first case, without regard to signs, was 14''.195, the mean being 0''.747; in the second case the corresponding sum was 11''.66, and the mean 0''.61; the latter measures fulfilling the conditions of the figure better than the former. Of the corrections presented by the theory of least squares, eight had a positive, and eleven a negative sign, and the mean taken with regard to signs was —0''.249. By the other method, nine of the corrections had a positive and ten a negative sign, and the residual error was —0''.16; so that in every respect the second series was superior to the first.

These results did not depend upon too small a number of observa-

tions being made, the corrections in triangles, of which the angles were measured by a larger or smaller number of observations, not presenting corrections in accordance with such a supposition.

The investigations would be pursued and extended, so as to be certain that the result could confidently be relied upon. As far as it had now proceeded, it went to show that the application of the theory of least squares to deriving the most probable values of the angles from the observations, did not present the advantages usually attributed to it. It would not be difficult to assign, in the nature of the observations themselves, physical reasons why this theory should not be applicable. The residual of lateral refraction was in reality very considerable in value.

Tables containing the particulars of the numerical quantities, of which the results merely are referred to in this abstract, were laid before the Section.

Prof. PEIRCE followed with a few remarks on the improper use of the theory of the least squares. He was opposed to the use of the doctrine of chances, on which this theory is founded, in solving any of the problems of nature, where nothing is governed by chance, but by fixed laws.

Dr. GOULD observed that objections to the theory ought not to be founded upon the results except of its application to non-homogeneous observations, that is, to observations not made under the same circumstances.

Prof. HENRY remarked, that on philosophical principles, he thought that the theory of least squares was extremely imperfect, being based upon purely mathematical conceptions.

REMARKS ON BOLTONITE. BY L. SAEMANN, of Berlin, Prussia.

I HAVE the honor to lay before you some specimens of a mineral, occurring only some thirty miles from this city, and having yet been up to this time an object of much controversy among the mineralogists. The name of this doubtful species is Boltonite, and its locality the limestone quarries at Bolton, a township in Worcester county, Massachusetts.

The limestone of this and many other places in this state belongs to the geological system of metamorphic rocks, and is intimately connected with the Gneiss formation, which covers so large a part of the New England States. Prof. Hitchcock's masterly description of this

and all the other geological phenomena of Massachusetts is so well known and appreciated in this country, that I could hardly say any thing new upon the general appearance of that stratum.

My principal purpose being to collect specimens, I was obliged to work several days in that quarry, and among others it was the Boltonite which I sought for with great eagerness, having been aware during a previous investigation in most of the public and private cabinets of your northeastern States, that there was generally a great uncertainty and difference of opinion among your best mineralogists and chemists, as to the real value of this species. All the information I could obtain was, that the mineral presented itself in form of various colored spots in the white limestone.

My only, and indeed very reliable guide, was therefore Mr. Dana's excellent "Treatise on Mineralogy," and I am happy to state, that in this, as in many other cases, it proved to be a very successful one. Prepossessed as I was of the idea that my species should be a very obscure one, I felt little inclined to recognize it in the very distinct if not very beautiful specimens of which the piece before you is a sample. Yet the description given by Mr. Dana proved to coincide exactly, as far as my very simple travelling apparatus allowed an investigation into the subject, and I felt happy to get a large lot of better specimens than I had ever before seen.

The mineral as it lies before you, is intimately associated with Scapolite, coating the interior of a large gap in the rock, the same when it occurs in limestone, being generally of far less beauty.

Knowing the compliance of Prof. Silliman, Jr., of New Haven, in all cases concerning the advancement of the science, I despatched immediately some specimens to him, submitting them to his judgment, and soliciting an analysis if the object should seem worthy of it.

Prof. Silliman, I am aware, will give you to-day the result of his investigation, and I doubt not his report will fill an interesting page in the history of mineralogy.

In the mean time I was not lazy in examining myself the specimens. The most remarkable character seemed to me to be the uncommon change of color, from dark greenish and bluish shades to a very distinct wax and sulphur yellow, as may well be observed in the specimens before us. I collected some specimens near the lime-kiln at Bolton, which, having passed through the fire, present a reddish brown color, and show thus a sort of blow-pipe reaction on a large scale.

A close examination with the magnifying glass proved first the presence of a black, very brilliant substance; having, as far as it can be ascertained on so small particles, the general appearance of titaniferous iron ore. There was no doubt that many of the specimens owed part of their dark color to this mixture, and I thought it possible that a higher oxidation of it could be the cause of a subsequent change of color. Nevertheless the immutability of this mineral under common atmospheric influences being well known, I hesitated again to adopt this explanation, and I was indeed finally fortunate enough to discover what I now am certain to be the real reason of this rather uncommon change.

A very minute examination of extremely small particles of the dark substance, disclosed the real texture of it. The principal mass, appearing entirely transparent and colorless, enclosed very small, sometimes dendritic scales of another mineral beside that already mentioned.

These scales when closely surrounded by the glassy mineral are of a brilliant metallic lustre, and of a dark yellow color; but commonly occurring between the laminæ and not entirely inaccessible to the action of air and moisture, they have a black or iridescent tarnish. They proved to be easily soluble in diluted hydroclorie acid, and from these characters, together with the effect of their decomposition, I conclude that they are magnetic pyrites.

Now the change of color in our mineral is easily understood; magnetic pyrites is susceptible of decomposition, under the action of atmospheric air and water alone, and certainly more so when in so high a state of division as is here the case. The protoxide of iron, or perhaps a sub-sulphate of it, resulting from this chemical change, gives the new color.

I was not able to ascertain whether the before mentioned titanate of iron undergoes the same change; it may be possible that the acid disengaged from the pyrites in its vicinity attacks and decomposes it; but even if this be not the case, its coloring power ought to be considerably diminished, the mineral now being opaque through the interposition of a yellow powder between its laminæ, and thus the titanate being unable to act in its most powerful way, that is, by reflexion through a limpid substance.

The accompanying specimens illustrate the progress of decomposition in all its different degrees. The microscopic evidence for my above statements will easily be obtained by those interested, more especially in this kind of observations.

In either case the constitution does not agree with that of any known species, and Boltonite must be regarded as a good species, should these results be confirmed by subsequent investigations.

The question now arises, what is the Bisilicate of Magnesia of Dr. Thomson?

Referring to Thomson's analysis and description of this mineral, we read (L. C. p. 50,) that the mineral received by him from "Mr. Nuttall, (from Bolton, Mass.) bears so much resemblance to the Picrosmine of Hadinger, both in character and composition, that he strongly suspects the two things to be mere varieties. The mineral is white, with a shade of green, powder white. It consists of a congeries of prismatic crystals, very irregularly disposed, and involved in each other. Lustre, glassy; transparent on the edges. The analysis gave:

Silica	56.64
Magnesia	36.52
Alumina	6.07
Perox. Iron	2.46
	101.69"

This analysis and description agrees neither with Boltonite, as above described, nor can we deduce from the analysis the constitution of Bisilicate of Magnesia.* We must therefore look for another mineral. Among the specimens from Bolton, in the Lederer collection, is one, No. 279, (1018*a*,) marked "Picrosmine?" "Actinolite?" This mineral answers the description of Thomson, quoted above, as nearly as any thing could, and is undoubtedly the same thing which he received from Mr. Nuttall, and examined with the above results. It consists of a congeries of prismatic crystals of a glassy lustre, and greenish color. Nothing else occurs at the locality, so far as can now be ascertained, at all resembling the mineral which is described by Dr. Thomson. A qualitative analysis of this specimen gave Silica, Magnesia, Alumina, Peroxide of Iron, Manganese, but no Lime or Water. These are the constituents of a hornblende, and this specimen is undoubtedly such — variety Actinolite.

I would say, in addition, that, regarding the Alumina in this min-

* A Bisilicate of Magnesia would contain 18.28 per cent. Magnesia, and 87.72 per cent. of Silica. It is quite impossible to understand what Dr. Thomson means, when he states that this analysis gives two atoms of Silica and one atom of Magnesia. It certainly is not so.

eral as taking the place of a part of the Silica, and the Iron to exist as protoxyd, (as it undoubtedly does,) Thomson's analysis gives very closely the normal formula of a hornblende, namely $\dot{R}^4 \dddot{Si}^3$ = Silica 62.1, Magnesia, 37.9.

If the foregoing conclusions are correct, it would appear that Boltonite is not the same mineral analyzed by Thomson, as supposed by Prof. Shepard, in his treatise, as quoted above: and also the so-called Bisilicate of Magnesia, of Thomson, is Hornblende.

On Boltonite of Shepard and Bisilicate of Magnesia of Dr. Thomson. By Prof. B. Silliman, Jr.

Prof. Shepard, in his Treatise on Mineralogy, in 78, (New Haven, 1835,) gave the name Boltonite to a massive mineral which is found at Bolton, Mass., disseminated in white limestone. Its structure is granular — or in cleavable masses of irregular outline, with one brilliant and two less distinct cleavage planes. Form probably oblique rhombic. In some cases, where the weather has dissected out the crystals from the calcareous gangue, the form is tolerably distinct, but not sufficiently so to admit of measurement.

Its lustre vitreous to sub-adamantine; its color greenish black and gray, changing on exposure to brown, wax yellow, and sometimes almost white. These changes of color are peculiar, and often the same mass which is dark greenish gray on one end, will have turned light yellow on the other. Hardness 5.50; specific gravity, 3.008 — the same on two specimens, one dark and one light.

This mineral, when first found, was called *Pyrallolite*, and is now so labelled in some old collections. For instance, Baron Lederer's Cabinet of American Minerals, now in the Yale College collections, contains eight or ten specimens of this mineral from Bolton, under the name Pyrallolite, which were received, as the catalogue indicates, from Robinson, Shepard, Nuttall, Boyd, and other of the early cultivators of American mineralogy in this country.

In his description of this mineral, Prof. Shepard says, it is believed to be identical with the substance described by Dr. Thomson, (Am. Lyc. Nat. Hist. New York, Vol. iii. p. 50,) under the name of "Bisilicate of Magnesia;" and accordingly the analysis of Dr. Thomson is quoted under "Boltonite," as the constitution of this substance.

It will presently be shown that it is in the highest degree probable

that Dr. Thomson applied his name Bisilicate of Magnesia to another substance, and that the Boltonite of Shepard is not the substance which he analyzed.

Mr. Saemann, the very discriminating and intelligent mineralogical traveller from Berlin, has lately visited Bolton in search of these minerals, and has had the goodness te send me three specimens of what he supposes to be Boltonite. These specimens are now on the table, and answer to the description given of that mineral in Shepard so closely that there can be no doubt that they are the same substance. If any doubt existed, however, it is quite removed by the specimens obtained from Lederer's cabinet. Mr. Saemann, in his note accompanying the specimens, inquires if the mineral may not be *Sphene*. Its behavior before the blowpipe appeared at first to confirm the correctness of this suggestion; but the reactions for titanium which were obtained in the blowpipe examination were due, undoubtedly, to a portion of titanic iron which occurs abundantly in very minute grains, and intimately mixed with the Boltonite. The dark colored portions of the mineral also turn light yellow under the blowpipe, a character which belongs to many brown sphenes, and, probably, to a large number of other minerals.

An analysis of the Boltonite gave the following results:

Silica	46.062
Alumina	5.667
Protox. Iron	8.632
Magnesia	38.149
Lime	1.516
	100.026

The formula which agrees with this analysis estimating the Alumina as a constituent part of the mineral, (which it undoubtedly is,)

Will be either $\dot{M}g^2\ (\dddot{Si}, \dddot{Al})$ or $\dot{M}g^2\ \dddot{Al} + 8\ \dot{M}g^2\ \dddot{Si}$.*

If the Alumina should prove to be an unimportant constituent of the mineral, and is disregarded in the formula, then the analysis gives the ratio of 4 : 3 for the relation of the Sesquioxyds to the Protoxyds, which ratio gives the formula

$$\dot{R}^9\ \dddot{R}^4 \text{ or } (\dot{M}g\ \dot{F}e\ \dot{C}a)^9\ \dddot{Si}^6.$$

* $\dot{M}g^3$ is put in this formula for $(\dot{M}g\ \dot{F}e\ \dot{C}a)^3$.

IDENTITY OF SILLIMANITE OF BOWEN, OF BUCHOLZITE OF BRANDES, AND OF FIBROLITE OF BOUMON WITH THE SPECIES KYANITE.

Sillimanite was originally described by Bowen — from an analysis made in Yale College Laboratory in 1824*; which showed it to be a silicate of Alumina with a proportion of silica too high to allow it to come within the formula of Kyanite. It was subsequently analyzed by Dr. Thomas Muir, in the laboratory of Dr. Thomson, who found in it a large quantity of Zirconia, an observation which all subsequent researches have failed to confirm. Since that time it has been frequently analyzed by various chemists, viz.: by Connell, Norton, Staff, Thomson, and Hayes. One of the most recent of these analyses which has been published, is that by Dr. Thomson, who reports it to contain 45.65 *per cent.* of silica. We have then the following discordant results in the amount of Silica found in Sillimanite by different chemists in the order of their publication.

	1.	2.	3.	4.	5.	6.	7
	Bowen.	*Muir.*	*Connel.*	*Norton.*	*Staff.*	*Thomson.*	*Haynes.*
Per cent.	42.67	38.67	36.75	37.70	37.36	46.65	42.60

The cause of this disagreement will undoubtedly be found in the difficulty of effecting a complete decomposition of those anhydrous silicates of Alumina, which contain a high per centage of Alumina. This decomposition can be completely effected only by the aid of caustic potash applied during the fusion, as first recommended by Berzelius.

Select crystals of this mineral were taken from the original locality at Chester, Conn., and their analysis afforded the following results: Quantity taken .7755 *Gramme*; found,

Silica	.292 = per ct.	37.653 = 1	or 2	
Alumina	.484 = per ct.	62.411 = $1\frac{1}{2}$	or 3	
	.776	100.064		

			Required.	*Found.*
2 atoms Silica . .	1154.62	= $\dddot{Si}$ . . .	37.47	37.753
3 atoms Alumina .	1927.00	= $\dddot{Al}$ . . .	62.53	62.411
	3081.62		100.00	100.064

This result gives exactly then the formula of Kyanite, viz.: $\dddot{Al}^3 \ \dddot{Si}^2$. The analyses of Staff and Norton give also the same result.*

* Am. J. Sciences, Vol. 8, 1 Series, p. 113, and Jour. Acad. Nat. Sci., Philad.

We can, therefore, have no longer any hesitation in referring Sillimanite to Kyanite, as originally suggested by Mohs, in his Mineralogy.

Bucholzite is a name given by Brandes to a Silicate of Alumina from Tyrol, which occurs in compact masses of a finely fibrous structure and hardness equal to Kyanite. Thomson also has analyzed a mineral from Chester, in Pennsylvania, well known to collectors, and has referred it also to Bucholzite. Being in possession of authentic specimens of this mineral, I have analyzed it with the following result. Quantity taken 0.561 *Gramme.* Found,

			1	2
Silica	.1925	= per	34.31	35.96
Alumina	.3615	"	64.43	
Magnesia	.0028	"	.52	
Manganese	trace.		trace.	
	.5568		99.26	

This also will give us the same formula with Kyanite. The mineral being less pure than Sillimanite, cannot be expected to furnish results as accurate as the former analysis.

There is also found at Brandywine Springs, Delaware, a mineral which has been extensively distributed among collectors under the names Bucholzite and Fibrolite. A specimen from this locality furnished me the following results, viz.: Quantity taken, 1.0675 *Gramme;* found,

$\dddot{Si}$	.386	= per	36.159
$\dddot{Al}$	.679	"	63.525
	1.065		99.684

This is, of course, identical with Kyanite. Minute traces of Iron and Manganese, which are found in both the above, are regarded as of no importance in the result, being mere impurities.

Fibrolite of Bournon. This mineral was first distinguished by Count Bournon, who detected it among the associated minerals of Corundum from India and from China. The name has reference to its fibrous character. It was analyzed by Chevenix, who found

* In Norton's analysis, which was made in Yale College Laboratory, the excess of 2.73 was owing undoubtedly to Aluminate of Potash which remained in the Alumina after separating the Peroxyd of Iron by Caustic Potash. Subtracting this sum from the sum of Alumina and Peroxyd of Iron, we have 62.30 per cent. Alumina and Peroxyd Iron, which is almost exactly the quantity required by theory.

Silica, ...	38.00
Alumina, ...	58.25
	96.25

Even upon so imperfect an analysis, there has been no hesitation in referring it to Kyanite. Having an authentic specimen of this mineral from Count Bournon at my disposal, I have analyzed it. It yielded on 0.427, *Gramme* taken, viz:

$\dddot{\text{Si}}$.	.1551 = per	36.309	$= \dddot{\text{Al}}^3 \, \dddot{\text{Si}}^2$.
$\dddot{\text{Al}}$	.2665	" 62.415	
$\dot{\text{Mg}}$.	.0030	" .702	
	.4246	99.426	

The results just given leave it no longer possible for us to separate Sillimanite, Bucholzite, and Fibrolite from Kyanite. The hardness of Sillimanite proves also to possess the same inequality on different faces which is found in Kyanite. The cleavage face is much softer than the angle or side of the prism, so as to be easily scratched with a sharp point of hard steel. The crystalline forms of Sillimanite and Kyanite are also identical, the one being derived by the simplest modification from the other. The cleavage in both is in the orthodiagonal.

It may be worthy of remark that "Andalusite" has the same chemical constitution as Kyanite, but belongs to the right rhombic form, while Kyanite is oblique. Doubtless this is a case of dimorphism, and perhaps the same may be said with truth of Staurotide.

Adjourned.

B. A. GOULD, Jr., *Secretary.*

August 15th — Afternoon.

GENERAL SESSION.

The Association met in General Session at half-past 4 o'clock. The first communication was by Prof. H. D. Rogers, on the Structural Features of the Apallachians, compared with those of the Alps and other disturbed districts of Europe.

In this communication the speaker confined himself to the analogies of structure which he had discovered between the Apallachian mountains of the United States and the disturbed mountain zones of Europe.

He first sketched the characteristic features of the Apallachians on their southeast slopes, where the strata are invariably doubled into oblique flexures or folds. Further towards the northwest, or central belts of the chain, these flexures are less perceptible, but the inverted or northwestern side of the anticlinal curves dips much more steeply than the southeastern. Advancing still further across the chain these great flexures or arches of the rocks progressively expand, the curvature of the northwestern slopes still, however, dipping very steeply, while in the broad plateaus of the Alleghany and Cumberland mountains the arches or waves subside and dilate into symmetrical undulations of equal and gentle curvature. Along all the southeastern border of the chain, the prevailing dip is therefore toward the belt of active igneous movement, where alone the strata are penetrated by intrusive volcanic rocks.

The speaker next showed that these arches or waves are of great length, and, whether straight or curved, exhibit a singular degree of parallelism and uniformity in their style of flexure. In the southeastern and middle zones of the chain, many of these great arches terminate in enormous longitudinal faults or fractures, which are nothing else than inverted flexures broken at some point in the inverted part of the anticlinal, producing the apparent anomaly of an overlapping of newer strata by others of far older date. Some of these fractures thus ingulf a thickness of nearly two miles.

The cleavage planes of the rocks are nearly parallel with the average dip of the planes which symmetrically cut or bisect the anticlinal and synclinal curves; and this law of position of the cleavage planes is found to prevail equally in the plicated districts of the Rhine and the Alps. The palæozoic region of the Ardennes and the Rhine was next described, and a section shown passing through the chain of the Ardennes and the coal-fields of Belgium. Precisely analogous features were proved to belong to this region, which had been observed in the Apallachians. In the more disturbed tracts the strata are closely and sharply folded into almost absolute parallelism, while further north, in the carboniferous basins of the Meuse, these flexures were shown to progressively dilate precisely as in the sections of the Alleghanies. It was further shown that the cleavage planes of the more contorted belt, are, as in the Apallachian region, parallel to the average dip of the axes, planes which divide the curves.

Prof. Rogers then proceeded to give a description of the picturesque and symmetrical mountain chain of the Jura, and proved that there likewise the same beautiful law of a peculiar curvature pre-

vailed, the great anticlinals exposing invariably, or, with rare exceptions, a much steeper dip upon the sides which face the Alps than upon the opposite sides. The average dip of the northwestern abutments does not amount to 40°, while that of the southeastern even exceeds 70°.

Alluding to the indication of a vast fracture or fault at the base of the southern wall of the Jura, the speaker showed how this break in the strata would explain, upon his theory of an undulation of the crust of the earth,—in this instance propagated, not from the Alps, but from the district of the Vosges, or the country towards the northwest—why the successive anticlinals, as we approach this fault, do not decrease in curvature, but on the contrary augment in steepness.

Passing next to the chain of the Alps, it was proved that it consists of two principal zones of closely plicated strata. The entire belt of the Bernese Oberland displays folds which dip inwardly toward the high central peaks, with a parallel or south dipping system of cleavage. The southern chain of the Monte Rosa exhibits a similar system of flexures, but of an opposite order of dips, these being directed toward the north, and, therefore, also inclining inwards towards the high central summits. This opposite direction of the folds in the two opposite flanks of the chain, at once explains the hitherto unsolved phenomenon of the inward dipping or fan-like position of the planes of stratification. The cleavage dips on each flank of the chain, as in every other district, are parallel with the average dips of the anticlinal folds.

Prof. A. Guyot expressed the satisfaction he experienced in seeing the elevation of the Jura, in its essential point of view, understood by Mr. Rogers as he himself had understood it, and publicly taught since 1840. He agreed entirely with Mr. Rogers when he attributed the elevation of the Jura, not to a force acting from below upwards, as is generally admitted, but to a lateral pressure. But, on the other hand, he did not believe that the point of departure of the upheaving force is to be looked for north of the Jura. The general orographic features of the Jura, and their relations to the Alps, made him think, on the contrary, that the immediate cause of the uplifting of the chains of the Jura is to be sought in the Alps, and that he must consider it as a contemporaneous effect of the repulsing action of the upheaval of the Alpine chain. Having been registered for a communication upon the same subject, Mr. Guyot requested permission to state on this occasion the facts which led him to this conclusion.

The principal chain of the Alps is E. N. E. The Jura, which at its point of departure touches the chain of the Alps, runs nearly N. E., and gradually diverges, so that the two chains form the two retreating sides of an acute angle. Now this disposition seems to determine the principal and characteristic features of the orography of the Jura, which are the following: —

1. The maximum of the height of the chain of the Jura is found near the point of contact with the Alps, and the elevations go on gradually and regularly diminishing in proportion as the Jura recedes from the Alps. The Cret de la Neige, near Geneva, at 5300 feet; the Dole, in the Canton de Vaud, 5100; the Chasseral, near Neuchatel, 4900; the Weissenstein of the Jura of Soleure, over 4000; and so on to the extremity in Argovie, the height of which does not exceed 2700.

2. In proportion as we leave the Alps, cutting the Jura in a transverse direction from the Swiss plain to that of France, we observe the same decrease of elevation. The first chain, the nearest and in front of the Alps, is the highest, and the summits gradually sink from 5000 to 2000 feet to the other border of this vast undulating plateau.

3. The particular chains of which the system of the Jura is composed, are not exactly in the general direction of the system itself, but slightly oblique in such a manner, that they tend to place themselves in parallelism with the chain of the Alps.

4. Although the system of the Jura, taken together, turns its abrupt slope towards the Alps, as was remarked by Saussure, the arches, at least of the first chain, the nearest to the Alps, are often pushed over in such a manner, that the opposite slope, or the N. W. declivity, is steeper, and the arch as it were crushed.

Thus the intensity of the phenomena of upheaval augments or diminishes in the different parts of the Jura, in proportion as the distance which separates them from the Alps becomes greater or smaller. It is the same with the tertiary strata, with the molasse, which occupies the space between the two chains. It presents wrinkles and elevations which are higher and more broken, the nearer they are to the foot of the Alps; it is sometimes found entirely folded over upon itself, and plunging under the Alpine strata.

These general facts place beyond a doubt the existence of an intimate relation between the two chains, and all seem to find their natural explanation in the repulsing action which must have been exercised by the elevation of the mass of the Alps upon the

neighboring strata. Mr. GUYOT conceived the upheaval of the mountains as being the result of a contraction of the terrestrial surface in consequence of the gradual cooling of the globe, and therefore, as being wholly due to the lateral pressures which must result from this contraction. In this particular case, he considered the Alps as the principal fold, pushed to a complete fracture, which reacts around it, and crowds back the neighboring strata and forms secondary folds, which are the chains of the Jura. What still further confirms this opinion, is the fact that the Jurassic strata of the Alpine system present everywhere the traces of a violent pressure, and are those which present the phenomena of the most extraordinary folding. We may then believe that this pressure was transmitted along these same strata under the molasse which covers them, and manifested itself anew, although weakened in the folding of the chains of the Jura.

Before being affected by the upheaval of the Alps, perhaps the Swiss Jura had the form of the Wurtemberg Jura, that is, the form of a plateau inclined to the south, while after the upheaval of the Alps the southern part, on the contrary, was found to be the most elevated.

An inspection of the strata deposited in the interior of the valleys proves, moreover, that the Jura has undergone since this first epoch at least three successive upheavals; the first before the cretaceous deposits, the second after the cretaceous and before the tertiary epoch, or that of the molasse; the third, the most considerable, after the deposition of the molasse, which would seem to indicate for the Alps an equal number of corresponding movements.

As to the possibility of harmonizing all the geological facts of Central Europe with the grand idea expressed by Mr. Rogers, of a common law which would bind together all the upheavals of strata comprised between the Northern Sea and the Adriatic, nothwithstanding the different ages and various directions, it is a question which demands a deeper examination, which Mr. GUYOT would not at present enter upon.

Upon the delivery of this communication, a short discussion ensued, in which Profs. AGASSIZ, GUYOT, and ROGERS took part. The two former gentlemen confirmed the description given of the geological structure of the Jura, and stated that they had themselves been long familiar with the general fact presented, that the side of each great anticlinal which faces the high Alps is considerably steeper than the remote side.

Prof. ROGERS mentioned that he had nowhere been able to discover in any of the works or memoirs of the Swiss geologists a statement of this generalization, and that he felt it due to himself to state that he had arrived at it through his own observations.

Profs. AGASSIZ and GUYOT rejoined to this, that they were aware that this view had never been published, but Prof. GUYOT had taught the fact for many years in his lectures, and had been in the habit of ascribing it to the action of some prodigious horizontal pressure propagated from the region of the high Alps, during successive upheavals of that lofty chain.

Prof. ROGERS then drew attention again to the important fact, that the folds in the strata of the Jura being steepest on their south sides, while those of the Alps are steepest on their north sides, the movement uplifting the former must have come from precisely the opposite direction of that which caused the latter.

Dr. LE CONTE suggested that the erosive action of water had been exaggerated, in consequence of no distinction being made between valleys formed entirely by aqueous action, and those in which a previously existing fissure was merely modified by the passage of a large volume of water. He suggested that the only test was to ascertain if the direction of the deep gorge coincided with the line of least elevation of the general face of the country, as that would be the line followed by a current flowing over the land before any valleys were eroded. If the water channel deviated from this line, it was to be attributed (at least in part) to a ready formed fissure into which the water burst by the giving way of a barrier.

ON THE CONNECTION OF COMETS WITH THE SOLAR SYSTEM. BY PROF. BENJAMIN PEIRCE.

PROF. PEIRCE stated that there had been a century of exact observation upon Comets, so that it seemed worth while to inquire whether we could not now ascertain whether they are component parts of the solar system, or strangers visiting us from other systems. He believed that the facts were sufficient to decide this question.

If any form of the nebular hypothesis was to be adopted, it was necessary to consider their origin. Upon that theory, the readiest way to account for their existence, would be to suppose them strangers to all systems, being produced from portions of the nebulous fluid left

between the spheres of stellar attraction. His own opinion was, however, that they are component parts of our system, and that the comets within every system belong to that system.

There were two classes of arguments which might be produced. The first arising from the nature of their orbits — from their not being hyperbolical. Of the hundred comets which had been carefully observed, and whose orbits had been accurately computed during the last century, not one had been shown to have a decidedly hyperbolical orbit. But if the comets do not belong to our system, one half of them, upon the average, ought to move in orbits decidedly hyperbolical.

He came to this conclusion upon the ground that our system is moving in space. The very point towards which we are moving had been determined, and very recently, in a paper upon stellar astronomy, by Struve, the deduction had been given of the very amount of the motion of this system in space. Its velocity was computed as about one-fourth of the velocity of the earth's motion in its orbit. It would amount to the same thing, to suppose the solar system to be placed in a stream of stars to which the comets belong, and the average velocity of which the comets would possess. Now, by the laws of motion, if the comet came into the solar system with no velocity at all, its orbit would be a parabola; but if its velocity was sensible, it would move in a hyperbola, the form of which would be exactly dependent upon the amount of this velocity. It had been shown by La Place that the direction does not influence the character of the form of the orbit, but that from the velocity alone, at a given distance from the sun, it can be determined. But if the comets did not belong to the solar system, there ought to be some of them with very remarkable hyperbolical eccentricity; so that the fact that there are no comets with hyperbolic orbits, seemed to be in itself almost decisive proof that they do belong to the solar system.

Another effect of this motion in space would be, that the comets would more frequently enter the system upon that side towards which we are moving; which was not found to be the case.

Dr. B. A. Gould had drawn his attention to a defect in La Place's argument, based upon the inclinations of their orbits. La Place had taken the orbits of the comets as actually seen, and tracing their inclinations to the plane of the ecliptic, had shown, according to the doctrine of chances, that there was no connection between them and the ecliptic. But he had made a very important mistake in his investigation, which had vitiated the whole result, and the truth is exactly the reverse of that which he stated. La Place had supposed that in an

even distribution of the cometary planes, there would be just as many at one inclination as at another. On the contrary, you would find that there would be an accumulation of planes perpendicular to the plane of the ecliptic. At his request, Dr. Gould had made a chart of the poles of the orbits of the comets, which very evidently accumulated towards the poles of the ecliptic, in the case of the direct comets, but, in the case of the retrograde, the accumulation was not so decided. He had then made a chart of the planes themselves, which he had drawn upon a projection, taken from the frustum of a hexagonal pyramid, as the nearest approximation to the hemisphere. In this it was evident that there was an accumulation of the lines near the ecliptic. There were two other systems of lines which could not escape observation; one passing very near the plane of the Milky Way, and the other very near the nodes of the Milky Way, crossing the former at right angles. This would seem to show that there were a few comets having a cosmical relation to the great stellar system to which our sun belongs.

[These charts were here exhibited to the Association.]

To show how much the argument was worth, he had followed La Place in the doctrine of chances. Still he would protest against using the doctrine of chances for the absolute determination of laws. We are in a world not governed by chance, but governed by law; and all that we can do with chances is to negative chances, to show that they do not belong to the system.

He then proceeded to show how, by following the doctrine of chances, he might have been led to an erroneous result upon this very subject. Suppose that the orbits had been distributed near two planes crossing each other at moderate inclinations on opposite sides of the ecliptic. By the doctrine of chances, he could have proved, a thousand to one, that this system of orbits belonged to the ecliptic, while there could not have been a single orbit very near the ecliptic. Therefore, starting with a wrong hypothesis of law, to ascertain what the true law was, he should have come to a result radically wrong. So chances could never be used to establish laws. No machinery of chances would ever dispense with thought and observation.

What then, he inquired, was the probability, that this accumulation of the orbits near the plane of the ecliptic could have resulted from a mere chance distribution of the orbits? La Place found it two to one, by using a wrong principle. He (Prof. P.) had separated the orbits into two classes, the direct and the retrograde, since it might be sup-

posed that the direct comets belonged to the system, while the retrograde did not. The probability against this being mere chance distribution, was 34 to 1 for the direct, 17 to 1 for the retrograde, and 71 to 1 for the whole together. The chance was 71 to 1, then, that there was an actual law, and that the comets really belonged to the system.

Then the question would be, how the comets were connected with the nebular hypothesis? for there were so many things in favor of that theory, that even when it was proved to be false, it was not easy to disbelieve it.

La Place had founded an argument upon the fact that there are about as many retrograde orbits as direct ones. He was disposed to deduce from that fact a result opposite to the fact itself, and to use this fact to prove that there are a great many more direct comets than retrograde. This he would illustrate by the very familiar case of meeting people in the street. If we meet no more than pass us, we know that we are moving with the stream. We only see those direct comets which pass us near their perihelion, while we see nearly all the retrograde. We must remember, however, that the earth's motion was more like crossing the stream diagonally, than going with or against it. If then there were more direct than retrograde, here was another evidence that they belonged to the solar system, and in favor of the nebular hypothesis.

If a comet had been sent out from the sun by expansion, when it came back it could hardly escape from falling again into the sun, even if thrown tangentially. If sent off from the planets — and La Grange had found the necessary force to be comparatively small — the inclination and direction of the orbits would be about what they actually are; with a tendency towards the plane of the ecliptic, and towards direct motion. The great difficulty was in making the force exactly sufficient to produce the parabola, or lengthened ellipse; for if it was more than this, however minute the excess, they would pass from the system. Leverrier has shown that the action of Jupiter upon Lexell's comet might have changed its orbit into the hyperbolic form. So with this excess of motion, they would pass into another system, and thus pass from system to system, until they would pass so near some planet as to have their orbit reduced to the parabola or ellipse.

One remark with regard to the direction from which they come. Out of 90 direct comets, there were 57 which came from the south,

and 33 from the north. The retrograde comets were nearly in the same proportion. Out of 94, there were 55 from the south, and 39 from the north. This difference, however, might simply be the accident of observation.

Adjourned.

E. N. HORSFORD, *Secretary.*

Third Day, August 16, 1849.

THE Association met in general session at 10 o'clock, A. M., in Harvard Hall. After reading the proceedings of the previous day, the Association proceeded to elect the following members nominated by the Standing Committee: —

Hon. AMOS LAWRENCE, Dr. WILLIAM LAWRENCE, AMOS A. LAWRENCE, Esq., HENRY BELKNAP, Esq., and CHARLES SUMNER, Esq., of Boston; JACOB H. WURTZ, and Dr. THOMAS ANTISELL, of New York; WILLIAM S. CLARK, Esq., of Easthampton, Mass.; J. B. ALLEN, Esq., of Springfield, Mass.; Dr. CHARLES W. SHORT, and Dr. SCHUMARD, of Louisville, Ky.; WILLIAM P. BLAKE, Esq., of New Haven; and OLIVER CARLTON, Esq., of Salem.

The following Resolutions were offered by Prof. PEIRCE: —

Resolved, That a Committee be appointed to consider the practicability of introducing a universal system of Scales and Standards for Scientific Measurement.

Resolved, That this Committee be directed to correspond with other societies in this and foreign countries, and report at the next meeting of this Association.

Resolved, That the Secretary of the Smithsonian Institute officiate as the Chairman of this Committee, the remainder to be nominated by the Chair.

After some remarks upon the subject of these resolutions by Dr. BOYÉ, of Philadelphia, they were unanimously passed.

The Chair, on the following morning, announced as associate members of the committee, Professors GUYOT, STANLEY, A. D. BACHE, and Lieut. MAURY.

The Committee of Mathematicians and Astronomers, nominated by the Chair to act upon the resolution offered by Dr. A. D. BACHE, in relation to the paper of Lieut. DAVIS, upon the subject of an American Prime Meridian, was then unanimously elected.

The Committee consisted of Prof. A. D. BACHE, Sup't. U. S. Coast Survey; Lieut. M. F. MAURY, Sup't. National Observatory; Prof. BARNARD, of Alabama; Prof. LEWIS R. GIBBES, of South Carolina; Prof. COURTENAY, University of Virginia; Prof. S. ALEXANDER, of Princeton, N. J.; Prof. FRAZER, University of Pennsylvania; Prof. ANDERSON, of New York; Mr. MITCHELL, of Cincinnati; Prof. STANLEY, Yale College; Mr. MITCHELL, of Nantucket, Mass.; Prof. LOVERING, University at Cambridge; Prof. SMYTH, Bowdoin College; Prof. WINLOCK, of Kentucky; Prof. COAKLEY, of St. James, Md.; Prof. CURLEY, Georgetown College; Prof. FOWLER, of Tennessee; Prof. PHILLIPS, of North Carolina; Prof. BARTLETT, of West Point; Prof. SNELL, of Amherst, Mass.; Lieut. C. H. DAVIS, Sup't. Nautical Almanac.

To this Committee, on motion of B. A. GOULD, Jr., Prof. CASWELL, of Brown University, was added.

The Standing Committee reported to the Association the reception of a memorial from the "MISSISSIPPI ASSOCIATION OF GEOLOGY AND STATE HISTORY," stating the organization and objects of their Society. The Association has been established for the purpose of developing the geological, mineralogical, and paleontological characters of Mississippi, and for collecting and disseminating such knowledge among the citizens of the State, as may aid in bringing about a Geological Survey of the State; and asks the coöperation of the American Association to advance its objects. The Committee earnestly recommends to this Association their unanimous approval of the objects of that Society, and as an expression of the cordial interest felt in its labors, the following Resolution: —

Resolved, That the American Association for the Advancement of Science cordially approves of the establishment of Societies for the development of the Geology and Natural History of the several States, and that a Committee of fifteen be appointed to memorialize the State Governments which have had no Geological Surveys, or which have had them suspended, on the importance and necessity of establishing or resuming them.

On the next day, by recommendation of the Standing Committee, the following gentlemen were unanimously elected members of this Committee: — R. W. GIBBES, M. D., of Columbia, S. C.; Pres. E. HITCHCOCK, of Amherst College; Prof. H. D. ROGERS, of Boston; Prof. L. AGASSIZ, of Cambridge, Mass.; Prof. B. SILLIMAN, Sen., of New Haven, Conn.; Dr. S. G. MORTON, of Philadelphia; Dr. C. T.

JACKSON, of Boston; Gov. J. W. MATHEWS, of Jackson, Miss.; Dr. G. TROOST, of Nashville, Tenn.; Prof. WILLIAM B. ROGERS, of Charlottesville, Va.; J. HAMILTON COOPER, Esq., of Darien, Geo.; T. ROMEYN BECK, of Albany, N. Y.; JOSEPH DELAFIELD, Esq., of New York; Prof. LEWIS C. BECK, of New Brunswick, N. J; Prof. JOSEPH HENRY, of Washington, D. C.

An invitation was received from Messrs. BOND for the members of the Association to visit the Observatory on either of the remaining evenings of the session.

An invitation was received from Mr. and Mrs. SPARKS, to meet at the President's house on Friday evening, at half-past 7 o'clock.

Adjourned to meet in Sections.

E. N. HORSFORD, *Secretary*.

Third Day, August 16, 1849.

SECTION OF GENERAL PHYSICS.

THE meeting being called to order by Prof. JOHNSON, Prof. A. D. BACHE was chosen to preside over the meeting for the day. Yesterday's proceedings of the Section were then read by the Secretary.

The first communication was presented by Prof. HORSFORD.

ON THE MOISTURE, AMMONIA, AND ORGANIC MATTER OF THE ATMOSPHERE. BY PROF. HORSFORD.

Moisture. The observations recorded of the moisture commenced on the last day of February, and were continued, with the interruption only of the Sabbath, until the 12th of April, and thereafter occasionally down to the 20th of July. They were hourly, and numbered on some days as high as eleven, and on others but three or four. They were accompanied by notes of the barometer, the temperature as indicated by the attached and external thermometers, the sky, whether cloudy and to what extent, and the direction and approximate force of the wind.

The method employed was that of Brunner, which consists of an apparatus for transmitting a known volume of atmospheric air through a chloride-of-calcium tube, previously and subsequently weighed. The difference between the weights before and after the experiment presents the amount of moisture in a given volume of air.

Among the results to which these observations have conducted, the following may be mentioned: —

1. As has been observed, other things being equal, the moisture is in general proportion to the temperature.

2. Slight variations of temperature are not accompanied by corresponding variations in the quantity of moisture.

3. Great variations in the quantity of moisture may take place while the temperature and altitude of the mercurial column remain constant. The quantity of moisture has even doubled in the course of an hour, although the temperature became reduced.

4. As a general remark, the moisture on the same day seems to depend chiefly on the direction of the wind.

5. The least quantity of moisture was observed during a N. W. or N. N. W. wind; the largest during a S. W. or S. S. W. wind.

6. The former occurred on the 12th of March, and the latter on the 23d of June. The quantity on the latter day was to that on the former as more than fifty to one.

The permeability of atmospheric air to aqueous vapor was a subject of experiment.

A half gill flask containing a small quantity of water was provided with a tube bent at right angles and connected by a cork covered with sealing wax. A similar larger flask, containing a small quantity of concentrated sulphuric acid, was provided with a similar tube, a foot or more in length, which was connected with the former by caoutchouc. The observation consisted in weighing from day to day the flask containing water, while the sulphuric acid flask remained at rest. The connection during the weighing was interrupted by releasing the caoutchouc tube from the water flask. The weight of the water diminished daily with a rapidity proportioned to the temperature. The suite of observations was continued through several months.

The "*striking through*" as it is termed, of ink employed in writing, has been observed to take place more promptly in very hot than in cooler weather.

A piece of writing paper of known superficial area was placed in a glass tube closed at one end, and weighed from day to day, noting at the same time the temperature. It was found to weigh more as the temperature was higher.

The quantity of Ammonia in the air was determined by an apparatus of the author's construction. The object in view in the arrangement of the apparatus was to provide that the air should, by means

of an aspirator, be transmitted through a constantly renewed atmosphere of hydrochloric acid vapor. To this end a series of tubes containing asbestos drenched with hydrochloric acid, and terminating in a potash bulb tube containing diluted hydrochloric acid, were connected with a safety tube, which was connected with an aspirator. Through this apparatus a known volume of air was transmitted. At the conclusion of the experiment, the apparatus was thoroughly rinsed with distilled water, and the Ammonia determined in the usual manner with bichloride of Platinum.

To find a normal atmosphere, two determinations of the Ammonia were made from the end of Foster's wharf, Boston, which in one direction looks out upon the Atlantic, during the prevalence of a strong east wind. A second determination was made in a court called the "Half Moon," communicating with Broad Street, Boston, an area of crescent form, about 180 feet by 70 feet, surrounded by brick buildings of five and six stories in height.

In these buildings it was estimated there were not less than a thousand individuals, most of whom, in extreme poverty, have within a few months arrived from Ireland. This locality was pointed out by one of the Assistants of the City Marshal, in reply to the request that he would direct to the worst habitable part of the city. In the centre of the court were the common vaults and sinks serving this large population. The atmosphere of this court, which was offensive in a high degree, was not found to be distinguished on account of its Ammonia, above that of the ocean in an east wind.

Two determinations were made in Cambridge, one during the prevalence of an east wind, the other during a west wind.

The quantities of Ammonia in the east wind, as ascertained from the above determinations, varied very considerably from each other. Great care was taken in the preparation of the reagents employed, and the Ammonia in the hydrochloric acid, water and bichloride of Platinum was previously ascertained. Still such was the discrepancy between the author's results, that he forbears a statement of the quantities ascertained, only so far as to remark, that they very greatly exceed those obtained by Fresenius in his recent determinations at Wiesbaden.

Organic matter has been observed in the air by several scientific men. Henry and Chevallier have detected acetic acid, and probably hippuric acid, in the atmosphere of a stable.

On transmitting a large volume of air through the Ammonia apparatus, supplied with hydrochloric acid, and evaporating in Platinum

over a water bath to dryness, and igniting the products of a thorough rinsing, no blackness was observed. On transmitting a similar large volume through a similar apparatus supplied with caustic Potassa, previously found to be free from organic matter, and treating as above, the residue blackened. This goes to show, as far as a single result may, that the organic effluvia in the air are of acid character.

An instrument was presented for determining the relative quantities of Ozone in the air at different times. It consisted of a tube, containing at one end a plug of Asbestos, moistened with a solution of Iodide of Potassium and starch. This plug within the tube attached to the aspirator, would, as air passed over it, become blue. If much water flowed from the aspirator, and of course much air flowed over the Asbestos, before it became blue, the quantity of Ozone indicated would be small. If but little water flowed (and this could be measured) the quantity of Ozone indicated would be greater. The quantities of Ozone would be inversely as the volumes of air passing through the tube before blueness is produced.

This communication gave rise to an animated discussion, in which Mr. Hunt, Professors Henry, Caswell, Boyé, Horsford, Hare, and others took part.

Prof. Henry remarked that he was much interested in the experiment of Prof. Horsford, in which vapor was shown to pass through a tube filled with air. It is well known that, according to the theory of Dalton, air and vapor are vacuums to each other. This theory is certainly in accordance with all the statical phenomena of the diffusion of vapor, but does not as well represent the dynamic effects. So great is the resistance to diffusion through a narrow tube, that Prof. Espy has concluded that the theory is incorrect, and that diffusion of vapor cannot take place without the aid of a current of air. Prof. Horsford's experiment proves that a diffusion does take place through a tube, but in this case the force of diffusion may be considered a maximum.

If the force is much less, the effect does not take place. Several years ago I placed a small quantity of water in a retort, and joined the beak of this to the open beak of another retort filled with air. The retort containing water was placed within a room kept constantly at a mean temperature of about 65°, while the body of the other retort was without a window, and constantly at a mean temperature of not more than 40°. Though the apparatus was suffered to remain thus, during a whole winter, not a single drop of water passed over.

The force of diffusion due to the difference of tension in the two retorts was, in this case, too small to overcome the resistance of the atoms to a passage between each other.

Prof. CASWELL, of Brown University, remarked that he had been much gratified with the paper of Prof. H. The question of moisture was one of great importance in Meteorology, and he begged leave to occupy the attention of the Section for a few moments in stating a few results of his own recent observations. The quantity of moisture, as indicated by his own observations, confirmed the statements of Prof. H. in not varying with the temperature. It sometimes remained stationary while the temperature varied through a pretty wide range; and it often occurred that the dew point *fell* while the temperature was *rising*. He recollected that on the 15th of July, when the air was remarkably dry, the dew point varied from 43°, at 7 A. M., to 42°, at 1 P. M., while the thermometer varied from 56° to 74°. On the 31st of July, when the air was very damp, the dew point varied at the same hours, from 69° to 72° while the thermometer passed from 74° to 87°. In the former case, the evaporation went on with extreme rapidity. The surface of the earth became almost as dry as if it had been baked in an oven; vegetation withered and suffered extremely; while on the latter, the evaporation was very moderate, and vegetation scarcely suffered at all from the heat.

The question of the dew point was highly important in explaining many meteorological phenomena. He had remarked, as doubtless all others had, whose attention had been directed to the subject, that this point was determinable with great precision. He had often observed that the deposition took place quite copiously at a given temperature, while at *one half* of a *degree of Fahrenheit* higher, no precipitation at all was perceptible. When the dew point and temperature were very near together, it required but slight reduction of temperature, perhaps hardly noticeable by the feeling, to fill the air with fog. Every astronomical observer must have often remarked the transition, sometimes in the course of a few minutes, from a clear sky to one entirely covered with vapor by this process of condensation, in a region of air but a little above him, where the temperature had fallen down to the dew point.

ON THE RELATION BETWEEN THE ELASTIC CURVE AND THE MOTION OF THE PENDULUM. BY PROF. PEIRCE.

ON this subject Prof. PEIRCE remarked, that the relations discovered merely by intellectual investigations, and not observed by the

senses, are of peculiar interest, as manifesting the fact that one intellect presides over the production of these phenomena. Could we see in the moon a house like our own, we should say that it was built by men like ourselves, having similar wants, and using similar means to supply them, and we should say that the same being who formed our minds created theirs also. We cannot make such observations, but we may trace relations between objects with which we are familiar, which lead us to similar results.

At present, the discovery of these relations has been very much confined to those subjects to which mathematics apply. He would call attention, at this time, to the similarity between the problems of the elastic curve and the pendulum. The external, sensible phenomena were very dissimilar, but intellectually they are the same, and the same principle is applicable to the solution of each.

The elastic curve is that formed by an elastic rod bent from its direction. The tendency of the rod to restore itself is proportioned to the amount by which it is bent from a straight line. The square of the velocity of the pendulum, when starting from a state of rest, is proportioned to the space through which it falls.

He then developed the equations derived from these principles, and showed them to be identical. The very same formula were applicable to the solution of the two problems, although different designations must be given, in the two cases, to the letters involved in the formula. The letters which denoted, in the problem of the elastic curve, the angular deviation from the primitive direction of the straight rod; the actual removal from the primitive position and the length of the rod were, in the case of the pendulum, respectively the angular deviation from the statical position, the velocity, and the time. The intellectual phenomena were precisely the same in both problems, but had a difference of material form, corresponding to this difference of notation.

The details of the external phenomena of the elastic curve and the pendulum, are perfect counterparts of each other in all their peculiarities. The pendulum cannot be made to vibrate in less than a given time, however small the arc through which it passes. But as you may make the elastic rod as short as you please, what resemblance is there to be found in the two cases? It is true that the rod may be as short as you please, but it will not be bent into the elastic curve with a given force, unless it surpasses a certain given length. According to the greater velocity given to it, the pendulum moves from a state of rest, vibrating in a greater arc, till at length it may be started so

rapidly, as to turn completely over, in which case the motion will always continue in the same direction. In the same way the elastic curve may bend back and forth in a tortuous course, or by a great increase of force it would pass round and round without any point of contrary flexure. But whereas the motion of the pendulum is perfectly monotonous, the variety in the forms of the elastic curve is curious and interesting. The straight line, the circle, and a form similar to the figure 8, are different examples of the elastic curve, and, intermediate between these simple cases, are other forms singular in their grace and apparent complexity, and not dissimilar to some of the patterns of borders of lace work. The case of the pendulum, moving with just that velocity which would carry it up to the state of rest, has its appropriate type in the elastic curve. The infinite time which would here be required for a single semi-vibration, corresponds to the infinite length of the elastic rod, which should extend from the point of contrary flexure to that of greatest curvature.

These remarks were illustrated by diagrams, which are necessary to develop their full beauty.

Chemical Examination of Gorgonia antipathes. By B. Silliman, Jr.

Since my researches on the chemical constitution of Calcareous Corals, which have been published in Dana's Zoöphytes, I have ever intended to extend my researches to that family of Zoöphytes having woody or elastic stems, and covered with a crust more or less calcareous.

Lately I have had an opportunity to commence an examination, not yet complete, on the stems of Gorgonia antipathes, of Bermuda. I present, at present, only a brief summary of the results already obtained.

The wood stem yields, on incineration, about 6 per centum of ash. A large part of this ash is soluble in water. The small remainder, insoluble in water, is entirely taken up by hydrochloric acid. Qualitative examination detected the following substances, namely, —

Potash, Soda, Lime, Magnesia, Iron, Chlorine, Bromine, Iodine, Sulphuric, Phosphoric, Carbonic, and Silicic acids. The reactions for Chlorine and Bromine, were very distinct in the aqueous solution, but Iodine could not be thus detected, because of the very large proportion of Bromine. It was, however, obtained by means of reaction with Bisulphate of Potash.

An approximate quantitative analysis yielded the following results. Omitting fractional parts.

Chlorine	14.0
Bromine	35.0
Iodine	13.0
Phosphoric acid	.3
Sulphuric "	3.0
Carbonic "	.3
Silicic "	.5
Potassium	1.6
Sodium	19.0
Magnesium	2.0
Lime	4.5
Besides Sand and Charcoal	3.6
	96.8

The proportions above given are only approximations, and need further confirmation by numerous analyses. We are struck at once by the very large proportion of Bromine, which, with the Iodine, covers nearly half the entire amount of the inorganic constituents. Unlike the stony corals, these woody ones have a very inconsiderable amount of Carbonate of Lime, and the Phosphoric acid is far less than might have been expected. The Fluorine could be detected in the ashes of this portion.* The exterior crust of this species will afford very different results from the stem now presented; and these, together with more complete analyses of the present subject, I shall hope to have the pleasure of presenting at the next session of this body.

On "Indianite" of Count Bournon, and on the American Mineral which has been distributed under the same Name. By B. Silliman, Jr.

This species, which has been associated with Anorthite by our best mineralogists, is a massive granular mineral resembling, in general appearance, a Dolomite, but easily distinguished by its great hardness. It forms the gangue of the Corundum in the Carnatic, and was first

* We are particularly struck by the very low amount of bases which yet remains to be accounted for.

distinguished by Count Bournon, who named it in reference to its oriental origin. Its color is whitish to red, or reddish yellow and gray. Hardness, 7 to 7.25. S. G. 2.668. It gelatinizes in acids completely. Before the blowpipe it is infusible. This species was analyzed by Langier and Chevenix, who found it to consist of

Silica	42.00
Alumina	34.00
Perox. Iron	3.20
Lime	15.00
Soda	3.25
Moisture	1.00
	98.45, Langier.

I was led to examine this mineral, in consequence of its close resemblance in physical characters to an American mineral associated with the Corundum of Pennsylvania, and which has attracted the attention of our collectors, who have distributed it under the name "Indianite." The result will show that the two things are completely distinct, although both belong to well known species.

The alkaline constituents were calculated on the results of a solution in chlorohydric acid. The Silica and other constituents were determined from an alkaline fusion.

Quantity taken 1.594 *Grm.*

Silica	42.09	.6710 = Oxygen ratio	21.869	4	= $12\,\dddot{Si}$
Alumina and Iron	38.89	.6200	17.610	3	$9\,\dddot{Al}$
Lime	15.78	.2516	5.292	1	$3\,\dot{R}$
Soda	4.08	.0651			
	100.84	1.6077			

The ratio $\dddot{R}^{12}$ $\dddot{R}^{9}$ $\dot{R}$ gives us the formula. $\dot{R}^{3}\,\dddot{Si} + 3\,\dddot{Al}\,\dddot{Si}$. This is exactly the formula for Anorthite.

The American mineral which has received the name Indianite at the hands of collectors, we shall now find to be quite another thing. This mineral is compact and granular, forming beds in chromic iron districts, and detached boulders in the corundum region. It is found both at Unionsville, Pa., and in many places in Lancaster County. It is remarkably tough under the hammer, and so hard, 7 to 7.25, as to offer a serious difficulty in the quarry. Its fracture is even crystalline, grains small, resembling Dolomite. S. G. 2.619. Completely unaltered in strong acids. Before the blowpipe it gives no

reaction for Soda; but this instrument detects in it, Silica, Alumina, Lime, and a trace of Iron.

Two analyses were made upon two specimens from different localities. For the Alkaline constituent, the mineral was fused with Carbonate of Baryta. The Silica was determined from a separate portion in the ordinary method.

First analysis of variety from Lancaster, Pa. Quantity taken = 1.234 *Grms.*

Silica . . .	.8225 =	per cent.		66.653
Alumina . .	.2565	"	"	20.786
Lime . . .	.0253	"	"	2.050
Magnesia . .	.0071	"	"	0.571
Soda . . .	.1155	"	"	9.360
	1.2269			99.420

Second analysis of a specimen from Unionville, Pa., sent to me by L. White Williams, of West Chester. Quantity taken, 2.180. *Grms.*

Silica . . .	1.4575 =	per cent.		66.857
Alumina . .	.4772	"	"	21.889
Lime . . .	.0389	"	"	1.785
Magnesia . .	.0105	"	"	0.481
Soda . . .	.1914	"	"	8.779
Water . . .	.0105	"	"	.481
	2.1860			100.272

It is unnecessary to educe a formula from these analyses, as it is obvious at a glance that we have only a granular Allite. The constituents of an Allite are:

Silica	66.11
Alumina	18.96
Protox. Iron	0.34
Lime	3.72
Soda	9.24
Potash	0.57
	98.94

Prof. S. also made a verbal statement, in reference to the optical properties of the Mica of Grafton, New Hampshire, and from other localities. He showed quartz compressed to a uniform thinness

between the laminæ of Mica, and also tourmaline compressed in the same manner, which showed beautifully under the light from a Nichol's Prism.

ANALYSES OF "GREEN PICROLITE," AND "SLATY SERPENTINE," SO CALLED, FROM TEXAS, LANCASTER COUNTY, PA. BY B. SILLIMAN, JR.

The analysis of these substances showed their identity with common Serpentine, and with the massive variety of the same mineral called "Williamsite," by Prof. Shepard. The small quantity of Alumina which they contain does not remove them from the formula of Serpentine when the results of the analyses are properly calculated.

The analyses gave the following result: —

	I. *"Green Picrolite."*	II. *"Slaty Serpentine."*	III. *Shepard's "Williamsite."*
Silica	44.250	44.58	45.40
Alumina	4.903	3.03	8.50
Peroyxd Iron and Protoxyd		6.15	
Protoxyd Iron	3.670		
Magnesia	34.000	34.51	33.60
Nickel	.690	traces.	traces.
Water	12.320	12.38	12.50
Mean of five analyses . .	99.833	100.65	100.00

The last analysis is quoted from Professor Shepard's account of his Williamsite, in *American Journal of Science, Vol. VI., p. 250, Second Series.* It was suggested that, in all probability, the Alumina in this last analysis contained a portion of iron, as it equals the sum of the Alumina and Iron in the other analyses. It is not easy to decide with precision how much of the iron in these minerals exists as protoxyd, but reactions for both are obtained in the "Slaty Serpentine." Serpentine can hardly be regarded as a simple mineral, and must vary in composition to a degree not allowable in a crystallizable substance.

GADOLONITE IN NEW YORK.

PROF. S. announced that the mineral found in granite in Monroe, Orange county, New York, and known as "Allanite," had given him the characters belonging to Gadolinite, which, as is well known, contains about 50 per cent. of the rare earth Yttria.

Remarks on Terrestrial Thermotics. By Lieut. E. B. Hunt, U. S. Corps of Engineers.

Few facts are established by more incontestable evidence, than that the temperature at the earth's surface was much higher in its early geological periods than at present. To account for this, on established physical principles, and without the use of precarious hypotheses, has not yet been so completely effected, as justly to preclude farther efforts. My present purpose is to state a mode of explanation, which seems to me more based on facts and conformed to Nature, than those hitherto suggested.

If we commence with the alluvial, and return chronologically to the primary formation, we find constant proofs that the earth's original atmosphere has been continually contributing to the solid strata of the terrestrial crust. The aggregate amount thus withdrawn, during the immense train of ages since organic life first appeared on the earth, is obviously very great. Oxidations and other chemical combinations, with the ceaseless processes of vegetable and animal nutrition, have been slowly but effectively bearing down the earth's gaseous envelop into its solid masses. The coal measures, especially, present an aggregate of matter originally atmospheric, which if now disengaged and restored to our present atmosphere, would very much increase its total amount or barometric indication. Coal beds, peat formations, humus or gëine, coral reefs, all carboniferous and fossiliferous rocks, have derived portions of their substance from the original atmosphere. If we suppose the history written in the earth's tables of stone to be reversed, until we reach those times, when tropical vegetation grew in circumpòlar localities, we shall find the corresponding atmospheric mass much greater than at present, and far more charged with carbonic acid. Whether our present advances in geology permit a moderately accurate estimate of the barometric indications, corresponding to the successive geological epochs, I know not, but a respectable approximation seems not unattainable.

Now the effect of a diminishing atmospheric mass would be a diminishing temperature at the earth's surface. Barometric readings of fifty and thirty inches would correspond to very different temperatures, just as we observe perpetual snow on mountain tops, overlooking plains always temperate or torrid. As we ascend in the atmosphere, the temperature constantly diminishes with the pressure,

abstraction being made of all disturbing causes. If we assume an empirical formula, with a barometric variable, to represent the variations of temperature experimentally determined, along a vertical through the present atmosphere, and apply it to the larger and heavier atmosphere of former epochs, the temperature at the earth's surface will be found to augment with the total barometric column, independent of all special hypothesis. It is observable that this increase will be uniform over the earth's surface, while the variable heat due to the sun's local actions must have been always essentially as it now is. Thus the climate of former periods would be more equable and uniformly torrid than at present.

In short, the ancient higher temperature of the earth's surface seems to follow, necessarily, from the simple facts, that the ancient atmosphere was more voluminous and heavy than the present, and that the temperature and pressure vary together. This view by no means conflicts with that of original incandescence, but seems to render it superfluous in this connection. It presents a cause apparently equal to the production of the effect, and free from that hypothetical and accidental character belonging to the theory of progressive cooling from an original fusion. The immense period of time which has elapsed since vegetation commenced on the earth, would seemingly have produced a far greater amount of terrestrial cooling than the observed, if the earth be regarded merely as a heated sphere in a cold space.

A special study of molecular forces and material constitution has presented to my mind the chief facts of terrestrial thermotics, under a beautiful and novel phase. The fact of a constant increase of temperature from the outer limit of the atmosphere to the earth's centre, by a rate increasing with the density of the stratum; what Fourier and Poisson call the heat of space; the former higher temperature at the earth's surface; these all follow by mathematical deduction from a few simple data, already generally accepted, and appear as direct static consequences, or as necessary parts of existing terrestrial equilibrium, not less than the tides or flow of rivers. The primary data are those of Franklin's electrical theory, as corrected by Epinus, and the resulting view of material constitution, somewhat like that presented by Mossotté, though he has failed to notice an important force of repulsion which, on these data, must act between adjacent molecules. In these discussions, the ether of physical optics enters largely; the molecules of bodies being indeed atoms with ether

atmospheres, which, in the composition of bodies, rest in those positions of equilibrium which result from the entire action of the primary forces.

Gravitation between separate masses thus appears the resultant of four distinct primary forces, all varying inversely as the square of the distance. These forces are so related, that particles of ether exterior to the earth, are subject to a direct attraction or gravitation towards its centre. It will be found that, if the data of Epinus, as used by Mossotté, be true, *ether must be ponderable;* a remark most prolific in consequences, but which I can now only state briefly, and without development.

Ether, being composed of mutually repelling particles, is an elastic fluid, and obeys the established laws of all elastic fluids. Ether and heat, or caloric, I shall regard as identical, having found a range of correspondences so extensive, simple, and uniform, that doubt has gradually vanished before them. The temperature at any point, is the elastic ether tension which would exist in a bulb, containing molecular vacuum when placed at that point. By treating ether or heat as a ponderable elastic fluid, the general facts of thermotics, common and terrestrial, are deductively explained in a simple and beautiful manner. Central heat, for instance, follows as a part of present terrestrial equilibrium, instead of being the residuum of a fusion, existing some myriads of years since, or, as Poisson has imagined, of the absorbed portion of a high heat in a region of space through which the solar system once passed. If we suppose the earth's mass now suddenly cooled to zero in all its parts, its gravitating force would rapidly draw in ether from space, until the present state of things should be restored. That which has been called the heat of space, is fully represented by an ether atmosphere which must exist, if ether be ponderable. This must extend much higher than the aereal atmosphere, blending finally with that stationary ether of space shown to be limitless by the transmission of luminous waves from the fixed stars. Its law of variation in density along a vertical, is transcendental, like that of the air. Now, if the data of Epinus be correct and universal, such an atmosphere must exist; and if it exist, it seems to me intimately connected with auroral, magnetic, and other meteorological phenomena. The currents due to the earth's rotation and translation, through the ether of space, are subjects of interesting speculation.

The ideas that ether is ponderable and identical with heat, seemed

at first to conflict with fact, experiment showing no dependence of the weight of bodies on their temperatures. But further examination showed that the quantity of ether displaced between the most distant temperatures we can command, is but an insensible fraction of the entire amount, entering into the constitution of any mass. The apparent conflict thus disappears.

The defects and contradictions of the views in physical thermotics, presented by La Place, Fourier, Poisson, Kelland, and others; their marked want of unity in one clear idea of what heat really is; the confessed insufficiency of every thing yet presented, to cover the whole ground of facts; and the great importance of arriving at more consistent and tangible conceptions on this widely extended subject, should certainly stimulate a more thorough study of the cardinal features of common and terrestrial thermotics than has yet been made. What is now needed, is a clearer insight into the primary nature of heat. The subject requires the aid of intelligent mathematical investigation, the first step of which must be the assumption of data. Reasoning will soon bring these either into conflict or harmony with established facts, thus disproving or proving their original correctness. The case is not appropriate to simple induction, but demands mathematical deduction from assumed premises. In assuming these premises, the simplest, and those best sustained by analogies and known facts, are most likely to prove true; this prestige belongs eminently to those of Epinus.

Neither should the mind be embarrassed with questionings about the materiality of ether. All that we can know of matter of any kind is, that certain forces act from, on, and in its ultimate units. This description applies to particles of ether, not less than to platinum atoms, as is shown by their action in luminous undulations and in resisting comets. I have found much gain in coming to a clear, simple conception of a particle of ether; seeing it as a *solid, defined volume, impenetrable, indefinitely smaller than an atom, possessing inertia, being both a source and recipient of incessant, radiant, emanative force.*

A medium, composed of such particles, is highly elastic, and follows all the established laws of elastic fluids; a form of law well known to apply to the conduction of heat. The study of a wide range of special cases and laws, convinces me, that heat, in all its phases and offices, is but this elastic ether, in rest, in flowing motion, or in transmitting undulation. Local temperature simply measures

local ether, density, or elastic tension. Heat, disengaged by pressure, is ether forced from the molecular atmospheres of the component mass, like water from a sponge. Conduction of heat is the flow of ether among molecules, like wind through a forest or pile of balls. Latent heat is the ether of molecular atmospheres confined by the attraction of the atomic nuclei. Radiant heat is ether transmitted by undulations in an ether medium; light being ether undulations of a different kind, unattended with actual translation of the vibrating medium.

In brief, by assuming the data of Epinus, exactly as used by Mossotté, we find, first, that a molecule is an atom with an ether atmosphere, very dense at its surface, and its radial decrease of density following a transcendental law. That the various phenomena of material aggregation or constitution admit of clear explanation by the combination of such molecules. That a force of gravitation, or one following the inverse duplicate law of variation with the distance, must always exist between separate masses so constituted. That if we identify heat and ether, the facts and laws of ordinary thermotics find a clear and simple explanation. That ether or heat is subject to gravitation, and that by so considering it, the grand facts of terrestrial thermotics at once result by deduction.

My chief present purpose has been to bring forward for the consideration of geologists, the question as to the aggregate amount of matter now in the earth's crust, which, through chemical agencies and the organic cycle, has been withdrawn from the original atmosphere. An approximate determination of the barometric indication on the earth's surface, at the dawn of organic life, if introduced into an appropriate formula of atmospheric temperature, will doubtless show for that epoch, a tropical temperature in polar latitudes. The question stated is simply one of fact, and possesses much interest aside from its proposed application. I may also remark, that the great subject of physical thermotics seems to have been, and to be too much neglected. Its thorough study now would surely do more to advance rational Meteorology, than the farther accumulation of weather observations without thread or connection. All must confess the great need of general guiding principles in these subjects; a need which can only be met by capturing this subtle fugitive, *heat*, in its obscure fastnesses, and clearly, steadily grasping the clue to its Protean nature. The whole groundwork of physical science is involved in that great problem of material constitution, to the complete solution of which

only can we confidently look for full insight into the thousand agencies of heat. This problem is to all the phenomena of inorganic nature what that of gravitation was to the movements of the Solar System, and must indeed include gravitation itself as part of its solution. Science has no need so great as a revival of the Newtonian spirit, and could the great "high priest" himself return, he would soon verify the belief expressed in his Principia, that all material nature is a problem in *mechanics*, an exponent of those forces belonging to the ultimate secrets of matter.

Adjourned.

B. A. GOULD, Jr., *Secretary.*

Third Day, August 16, 1849.

SECTION OF NATURAL HISTORY, &c.

Morning Session.

President Everett was appointed to the chair.

Prof. Agassiz presented to the Association living specimens of *Cyania fulva*, a new species, from the coast of Massachusetts.

On the Circulation of the Fluids in Insects. By Prof. Louis Agassiz.

The manner in which fluids are circulated in the body of insects has been a question long discussed, but never fully understood among naturalists. They have not precise material information upon the subject. Views, rather than facts, are presented in our works.

The organs of respiration have been well known to consist of air-tubes, opening outside and dividing within the body, thus circulating the air throughout the parts. Such air-tubes have been found, not only within the general cavity of the body, but also in the organs of locomotion, and even in the jaws and in the feelers of the head, so that all parts are provided with portions of the respiratory apparatus. But how the fluids derived from digestion are prepared by the alimentary canal to nourish all parts and provide for all functions of the body; how these fluids are acted upon by the air that is introduced in the body; how these fluids are circulated in all parts, was not understood. What has been known, up to this day, is the fact that there is an elongated vessel in the back of insects called the dorsal vessel,

which pulsates in a regular manner, and is a kind of elongated heart, divided into chambers, which emits fluid by branches from its anterior extremities, and receives fluids from the main cavity of the body through lateral openings. But the manner in which this fluid is circulated in the body could not be traced.

Prof. Newman, of London, was the first to ascertain that these fluids followed regular currents, regular tracks, along furrows in the cellular tissue of the body, and along the walls of the body, and that along these currents the fluid was brought back to the dorsal vessel. In this state of our knowledge of the circulation of insects, the respiratory organs appeared like ventilators, or air-tubes, plunging into the tissues, and thus providing them with air, during the passage of the fluids in various parts of the cavity of the body. Mr. Blanchard has recently made a very important addition to our knowledge of facts respecting these phenomena, namely, he has discovered that on filling the dorsal vessel of insects with colored matter it would follow the tracks of the tracheæ, or air-tubes; that the tracheæ would be universally colored; and that the fluid introduced follows the space between the two membranes of the tracheæ and the spiral threads which keep them apart. The tracheæ are known to consist of a double membrane, and of a spiral thread around and within the membrane. Now the colored matter introduced into this apparatus follows the path of the empty space, and the air which passes through the inner tube is thus brought in contact with the blood for the whole extent of the tracheal ramifications. Here it is at once seen that the contact is much more extensive than was suspected at first, and that the reaction between the air and the blood must be much more intimate than we could have supposed before the arrangement was known, by which the fluid is brought into contact with the respiratory vessel.

In repeating the observations of Blanchard, I have satisfied myself, first, of the perfect correctness of his statements; and I may say, of the great ease with which these vessels may be filled. It requires only the introduction of the injecting syringe with the colored matter into the dorsal vessel; and the best to select is indigo diluted oil-paint with pure turpentine, which makes a dark color, though the materials are so reduced as to penetrate the most minute vessels. By introducing it into the body of a living animal, you will see it at once circulated, almost instantaneously, into all parts of the body. It is no matter whether the liquid be introduced into the dorsal vessel itself, or whether it be introduced into the cavity of the body only; just as fluid

might be introduced into our body through a wound across the walls of the abdomen. In opening the insect after such an injection, all parts are found to be colored; — the tracheæ of the abdomen, thorax, jaws, legs, and feelers — all have their walls colored in blue. Now it was a point of interest to repeat these observations, and to ascertain the perfect correctness of these admirable and unexpected results in the discovery of Mr. Blanchard. But is there not something more to do in the matter and to examine how these vessels terminate?

When Harvey discovered the circulation of the blood — when he ascertained that the arteries were not air-tubes, as had been supposed, but vessels containing the blood in the body — when he ascertained that the heart was the central point of circulation, and that in the motion, the arteries carried the blood from the heart through the body, while the veins returned it to the heart — for all that was ascertained by his first experiments — he did not ascertain the manner in which the artery passed into the veins; the manner in which the blood of the arteries, carried to the parts most remote from the heart, turned back to trace a retrograde course, and again to reach the heart. The communication between the extremity of the arteries and veins, the whole system of capillaries, of tubes so thin that the fluids are no longer dependent upon mechanical power from behind for their motion, but upon the proper capillary action of the tubes themselves; — all this remained to be discovered; it has been discovered since the first discovery of the circulation itself.

So it is with this discovery of the circulation in insects; the manner in which the circulation takes place in the remote terminations of the vessels remains to be investigated. Upon this point I have devoted some attention; and to my surprise, I have found that the tracheæ are not all of the same kind; that we must distinguish between two sorts — respiratory tracheæ, I may call them, and circulatory tracheæ, or tracheæ for the circulation of the nutritive particles. The true or respiratory tracheæ terminate in bags, which, at first, would seem to be simply small vesicles, but which, upon close examination, under a high magnifying power, appear like diminutive lungs, scattered throughout the body consisting of cells over which the last branches of the tracheæ terminate, as the capillaries of the lungs terminate upon the air-cells of our respiratory system. An additional fact is the disappearance of the spiral thread and inner membrane of the trachea from the time the vessel enters into this kind of lungs, which it terminates.

[Prof. A. then exhibited a drawing, under a power of 700 diameters, showing this arrangement.]

The other kind of tracheæ, are those which I would call circulatory tracheæ, and which occur in the parts which require to be nourished by the blood. These tracheæ have no such air-sacks or cells appended to them. They are simply tubes, which branch, and subdivide, and ramify gradually into more minute, and finally terminate in the most minute vessels. And in these again, the spiral thread which characterized the trachea, disappears in the last minute branches of the tube, which I may call the capillaries of the trachea. These circulatory vessels divide, like arteries, in all parts. In the grasshoppers, for instance, which I injected from the dorsal vessel, I found the muscles within the legs, beautifully covered with dendritic brushes of these vessels, all injected with colored matter; and, in a highly magnified portion of a muscle from the leg of a *Acridium flavorittatum*, I have observed this (showing the diagram) distribution of these minute vessels, which will remind all familiar with the phenomena, of the striking resemblance there is between their distribution and distribution in blood-vessels in the body of higher animals.

I may add that, in these animals, I could not observe, what is so common in other animals, anastomoses — that is to say, connections between the terminal divisions of the vessel, forming a net-work, in which the fluid would move back and forth, as it takes place in the higher animals. I am inclined to think, although I could not see it, that the terminal vessels empty their contents directly into the cellular tissue of the body.

Observations on the Mirage seen on Lake Superior, in July and August, 1847. By C. T. Jackson.

The phenomena of Mirage have at all times excited the wonder and admiration of mankind, and have been fruitful in strange superstitious legends.

It is not surprising that

> —"the poor Indian, whose untutored mind
> Sees God in clouds, and hears him in the wind,"

should view with superstitious awe those strange and fantastic apparitions which manifest themselves in the wonderful Mirage which so frequently presents itself on the coast of the great lake to which I now call your attention. The Great Manitou of the Indians appears

to them in a great variety of forms, and his voice is heard in the rumbling thunder that echoes from the mountains bordering the lake shores.

Even those most versed in the causes of natural phenomena cannot fail to be strongly impressed with the magnificent phenomena of Mirage on the north shore of Lake Superior, and the philosophical mind delights in being able there to observe the causes which produce this marvellous effect. I know not whether the season, when I had the opportunity for making my observations, was one remarkable for the frequency of Mirage, but it is certain that for many successive days, the phenomena were presenting themselves in rapid succession along the northern coast of Lake Superior, opposite to Isle Royale, and on the coast of the island itself, in the bays which so deeply indent its shores. At Rock Harbor, on several occasions, I observed the little islands and points on its outskirts most perfectly represented with inverted pictures of their entire forms, hanging over their summits, the images of the spruce and other trees which crown them, being seen with beautiful distinctness directly over their terrestrial originals, while the picture of a little skiff was one day seen represented beside the phantom island, the boatman in the sky appearing to row his bateau as unconcernedly as his original on the bosom of the lake.

I endeavored to sketch this singular scene, but found it so fitful in its changes, as to render the task by my unskilful hands impossible.

On the 27th July we saw Keweenaw Point in Mirage. It is forty miles distant from this place, and bears E. N. E. from Scovil's Point on Isle Royale.

The most wonderful Mirage was observed from the north coast of Isle Royale, while we were coasting along from the eastern to the western end of the island. For several days in succession, we had almost hourly magnificent repetitions of these curious phenomena. On the 3d of August I took particular notes of the metamorphosis of Thunder Cape, which was subject to the most remarkable changes as viewed inverted in the air. A diagram may give some idea of it, but it would be impossible to represent its varied appearance, except by instantaneous daguerreotype impressions.

The islands on the north shore, called the Paps, were also seen inverted in Mirage.

Thunder Cape, 15 miles distant to the north, a lofty mural precipice, said to be 1300 feet high, and rising directly from the lake,

presents the form of an irregular truncated pyramid. By the phenomena of Mirage it suddenly changes its form into a huge anvil, sending out a long horn to the right, while a dark black mass rises behind it, which might be represented as old Vulcan himself. This singular phenomenon attracted much attention, and on observing with care, I found that the horn of the anvil was the image of the talus of the cliff on the shore, represented in inverted picture. The image seen at the summit was probably that of a conical peak in the rear of the cliff, represented inverted over the cape.

Turning away from this phantom for a while, when we looked again the anvil horn had been removed, and the figure over it was gone; but it soon reappeared as before, and for several days we were gratified with a view of these singular and interesting appearances, which seemed like the changes of the magic lantern. Occasional rumblings of distant thunder came to us from afar, though no storm visited us.

Not among the least curious and important refractions are those produced on the rays from the celestial bodies. At times the sun yields to the strange refractions, produced by the atmosphere over this great lake, and as he draws near to the horizon, expands his broad cheeks most good-naturedly, or sends out a long pear-shaped neck towards the horizon. Dr. John Locke took many sketches of the remarkable forms assumed by the sun, and will probably give some account of his observations. The afternoon observations for a time were found to be much affected by the unusual refractions of the atmosphere of the lake, and evening observations of the stars were found to be utterly useless. Only stars of very high altitude, such as could not be reached by the sextant with an artificial horizon, can be employed for determination of latitude and longitude. This was proved by numerous trials.

The morning observations were found to be more reliable, and were exclusively used in our determinations of longitude. It is probable that this extraordinary refraction is limited to the vicinity of the lake. It may be worth while to endeavor to explain the curious phenomena which I have described, and to account for the strange antics performed by the woodland scenery of the lake coast, and of the inverted image of the fisherman's boat as observed.

Lake Superior being an inland ocean of fresh water, in a high northern latitude, (between 46° and 49° north,) has a nearly uniform and constant temperature, probably not far from the mean tempera-

ture of the climate. It ranges from 37° to 42° F., never rising above the latter temperature excepting in shallow places near shore. The average depth of the lake is estimated by Bayfield at 900 feet. Its height above the sea is 600 feet, hence its bottom is 300 feet below sea level.

The shores of this lake are much more elevated than those of the other great lakes, and high table lands extend far back into the interior and are thickly wooded. The coast, especially on the north side of the lake, is abruptly precipitous directly to the water's edge. And the air on the surface of the lake rarely is of a higher temperature than 50°, while that in the forest at noon is frequently as high as 90°, or even 94°. It is obvious, then, that during a summer's day the air in the forests becomes highly rarified by heat, and takes up a proportional quantity of water in the state of invisible moisture. When this current of warm air slides from the precipices over the surface of the lake, the warm air by its specific levity from rarification floats upon the cooler air of the lake, and does not directly mingle with it. The consequence necessarily is, that a *film of moisture* is condensed at the surfaces of contact of the warm and cold air, and thus a screen is produced on which the objects reflected from below are seen as in a mirror. Meanwhile, by refraction, this image is seen higher up than it is really painted on the mist. This was obviously the cause of the strange phantoms which we have witnessed on Lake Superior.

It is no uncommon thing on other parts of the lake to see vessels inverted in the air before their hulls become visible above the horizon ; and it is well known that similar appearances *very rarely* occur on our seacoast, and have given rise, in former times, to strange and superstitious tales.

Prof. Agassiz mentioned an additional phenomenon which has been frequently witnessed by himself and his party upon Lake Superior. Not only did the shores and islands, with all their vegetation, appear repeated, higher up and in an inverted position, but that above this inverted landseape, there was sometimes still another, in which every thing was upright, so that the picture was twice repeated above the surface of real nature — once inverted, and above that, the same erect. This fact must be explained by any theory which professes to account for similar phenomena ; but it may be simply the image of the landscape, inverted upon the surface of the lake, reproduced with the inverted image of the landscape itself.

Dr. JACKSON remarked that he had observed the same thing frequently; but, by some oversight, he had made no minute of it.

Prof. ADAMS exhibited specimens of polished shells, showing that they were susceptible of a very high polish, by which their colors would be brought out, and they would be much more ornamental.

REMARKS ON THE DISTRIBUTION OF THE TESTACEOUS MOLLUSCA OF JAMAICA. BY PROF. C. B. ADAMS.

THE great number of species is remarkable. A few miles of coast, without the aid of storms, and without dredging, yielded 420 species. In the small bay of Port Royal, 350 marine species were found. A pint of sand, taken from a surface three yards long, contained 110 species. In Manchester, a district four miles by one mile, contains 100 terrestrial species. A small part of the island afforded 220 species of land shells. Probably there are 350 or 400 species of land shells, and two or three times as many of marine species. Extensive districts occur, however, which are nearly destitute of land or marine shells. They are accumulated in favorable stations.

The difference in the extent of the distribution of the marine and of the terrestrial species is remarkable. A majority of the marine species are known to occur in the other islands; probably not more than 10 or 15 per cent. of them will be found to be peculiar to Jamaica. But of the land shells, 95 per cent. are peculiar to the island.

The limited distribution of the terrestrial species is remarkable. A few are generally distributed, but a large number are limited to districts of a few miles in diameter, and several, although occurring abundantly, could be found only within the space of a few rods.

Only 17 fresh water species were found. Seven belong to Planorbis, and the other ten to nine genera. There are no Naiadae, and but one very small and rare Cyclas was found. Favorable stations for fresh water species are rare.

In respect of the number of individuals of Mollusca in Jamaica, as compared with more northern latitudes, the rule so obvious in the class of fishes is not applicable to the same extent. Of fishes the species are much more numerous, but the individuals much less so. Of the Mollusca, the total number of individuals is about the same as in this latitude, and the number of species represented by a profusion of individuals is about the same. But the number of species not

occurring abundantly is much greater, so that the average of individuals to all the species is less than in this latitude.

From a comparison of the laws of distribution of the marine and terrestrial species in the Antilles, it follows that the number of the latter must exceed that of the former.

With the insular distribution of the terrestrial species may be associated the fact that the coral reefs are all fringing, for both facts are connected with the geological fact that these islands are in a process of elevation.

On the River Terraces of the Connecticut Valley, and on the Erosions of the Earth's Surface. By President Edward Hitchcock.

I ought perhaps to say that my paper consists of suggestions and a few facts, or numerical results, rather than any finished theory. In fact, I follow my field notes chiefly, which have not yet been completed or reduced. I had hoped, ere this time, to present a carefully prepared paper upon these subjects, having been interested in them for twenty years, although I have never, until the past year, carefully measured the height of these terraces. I will be as brief as possible, as my leading object is, to elicit from others deductions from the facts which I have observed; and hope I shall be pardoned for presenting the subject in so crude a state.

I have examined the Connecticut River almost from its mouth to Turner's Falls; having carefully measured the heights of the terraces. But these measurements I shall not introduce, in detail, till I have carried the work farther towards the source of the river, which I hope to do soon.

During the last year, a work of considerable size has been published by Mr. Chambers, of Edinburgh, who considers most of the terraces in Great Britain as ancient sea-margins, which have become elevated to their present position by successive uplifts of the continent. Now, I suppose that our terraces are of three kinds; 1. The ancient sea-margins; 2. The margins of ancient fresh-water lakes; and 3. River-margins.

I have undertaken to examine the terraces of the Connecticut valley, which extends some 300 miles, nearly north and south, embracing the Connecticut and its tributaries. As you approach the river, you find plains of sand, gravel, or loam, terminated by a slope, sometimes as steep as 35°, and a second plain, then another slope and

another plain, and so on, sometimes to a great number. Sometimes, upon the other side of the river, you can perceive similar terraces, which seem, at that distance, to correspond in height with those upon which you stand, but which is not exactly the case.

I find that these terraces occur in successive basins, formed by the approaches of the mountains upon the banks, at intervals. Sometimes the basin will be 15 or 20 miles in width, but usually, much narrower; and it is upon the margins of these basins that the terraces are formed. The first basin which I examined, extends from Middletown northward to Mount Holyoke, where there is a barrier through which the river seems to have cut. North of that you have another basin which extends from Holyoke to Turner's Falls; another which extends to Brattleboro; another to Bellows' Falls; another to Charleston; another to Windsor, &c.

On the tributaries of the Connecticut River are several very fine terraces, and especially upon Deerfield River and Westfield River are remarkable ones which will throw a great deal of light upon the manner in which these terraces are formed.

I have rarely found terraces more than 200 feet above the river; which would be, in Massachusetts, about 300 feet above the ocean, and at Hanover, New Hampshire, about 560 feet. Nowhere do they exist, along any river, unless that river has basins; unless the mountains close down upon the river, and it has cut through the barrier at some time or other. In such basins, if they have considerable width, you will find terraces. As to the materials of which they are formed, they appear exceedingly artificial. The outer, or highest terrace, is generally composed of coarser materials than the inner terraces. They are all composed of materials which are worn from the rocks; but the outer terrace oftener is full of pebbles, some of them as large as 12 inches, while the inner seem reduced to an impalpable powder, like the soil of a meadow which is overflowed during high water.

Whence did these materials originate? How did these valleys fill up to the height of 200 feet in many places? It must have required an immense amount of materials, brought in by some agency or other; for the materials were first worn from solid rocks, and afterwards brought into these valleys. The outer terrace appears to have been often in part the result of the drift agency. Afterwards, the river agency sorted the materials, and gave them a level surface, the successive basins having at that time barriers. The inner terrace appears to have been, at least, in its upper part, the result of deposi-

tion from the river itself. It is composed of fine sand and clay, or loam. Such, for the most part, I conceive to have been the origin of the materials.

I will now mention a few of the facts which I have observed.

First, the terraces do not generally agree in height upon the opposite sides of the valley. I do not find any such correspondence, except in the case of deltas at the mouth of the smaller streams. The higher ones oftener agree, perhaps, than the lower ones. To an eye unaccustomed to judge of heights, a difference of 20, 30, or 40 feet, may be imperceptible at the distance of a mile or two; but measurement corrects the error. If formed, as I suppose, from the rivers, we should not expect them to agree in height upon the opposite sides of the river.

Another fact is, that the terraces slope downward in the direction of the stream. The same terrace which, near South Hadley, is 190 feet above the river, slopes until, at East Hartford, it is only 40 feet above the river, thus sloping 150 feet more than the slope of the river itself, in a distance of 40 or 50 miles. On the smaller rivers there are terraces which slope so rapidly that you can see it distinctly. This is a very important fact, as it shows that they cannot have been formed by the sea; for, in that case, they would have been horizontal; nor could they have been formed by a lake, for the same reason. It proves then, that they were formed by the river. The greatest number of terraces which I have observed upon the rivers is eight or nine; generally, there are but two or three.

I agree with my friend, Mr. Dana, who has published some very able articles upon the terraces, in the American Journal of Science, in almost every thing that he has advanced. I agree with him in the position which he has taken against that of Mr. Chambers, that we must require some decisive evidence in the materials besides their arrangement of a marine origin, before we adopt the conclusion that the sea produced them, and deny the agency of the river. Now in this whole valley of the Connecticut, 300 miles in length, in all these deposits, not a single organic relic of any kind has yet been discovered. The only things that look like organic remains, are two kinds of concretions; the most remarkable of which are clay-stones, which I believe the geologists of the north of Europe have considered as originating in molluscs, with which opinion I do not at all agree; they are formed, I think, as crystals are, rather than as the petrifaction, by which I mean the substitution of mineral for animal matter. I do not

deny that all this portion of the continent has been below the ocean for an immense length of time, and that it has been raised up by successive uplifts, and, probably, ever since the drift period; but the materials of the terraces in this country afford an evidence of such vertical monuments.

In 1833, when I made my first report of the Geology of Massachusetts to the government, I introduced my view of the manner in which the terraces were formed, which still appears to me the true one.

Let us suppose a basin, or lake, in the valley of the Connecticut, from Turner's Falls to Mount Holyoke, where there was a barrier. The highest terrace was then formed, the materials being brought in by the rivers from the surrounding country, and accumulated until the sand and clay had risen 200 feet above the surface of the present river. The barrier partially breaks through, and the highest terrace becomes dry. Still, when the river is swollen with rains, as it is two or three times in a year, it tears away its banks upon one side more than upon the other, according to its curve, or other circumstances, and thus forms a meadow, which is often overflowed and raised higher, by deposits, growing wider and wider. At length the channel would get so low, that even in its floods the river would be unable to overflow this meadow, and thus the meadow becomes dry land. Then it begins to cut away for a new meadow, and makes the former meadow a terrace. Thus the terraces are formed from the meadows, as the river sinks by the wearing away of the barrier. The work may go on with extreme slowness, and yet these terraces will be formed. Even now, new terraces are being formed in the same manner. You would not expect the terraces to be of the same height upon the opposite sides of the river, because they would not generally be formed simultaneously.

This appears to me to be one of the modes in which river terraces were formed. Some of them were probably produced in other modes, which time does not allow me to consider.

I hope also to show from these facts, how the conical and irregular mounds of sand and gravel which occur so frequently, and which have been called *moraines* sometimes, have been produced. But I do not feel prepared to go into the subject at present.

I have found several channels in the Connecticut River, long since disused, and which are shown on the map before you.

But the most remarkable changes in the bed of a river are seen in Deerfield meadows, where several ancient beds are obvious.

Rivers change in two ways. One is by wearing away one bank, and accumulating on the opposite side of the river. In thirty or forty years, I have found from this source a change of 50 or 60 rods. By such an action terraces could have been formed. Another mode of change is, by passing round an obstacle at once, for example, during a flood, when the usual channel is blocked up by ice and worn before the obstacle is removed, that the water continues to go there. The water sometimes breaks through, so as to make a short cut instead of a long curve, as it has recently done at the foot of Mount Holyoke, in Hadley.

I will now take up the subject of erosions upon the earth's surface. We are often amazed to see the large amount of loose materials spread over the rocks, and wonder where they came from. But if we will examine some spots where rivers have worn out for themselves gorges through mountains, we shall no longer be surprised at the immense amount of loose materials. The materials look as if they were worn away from the solid rock; but can we prove it? I think I have found some proofs which have been very much overlooked.

The deep channel which the Niagara has worn for itself, in the retreating of the falls several miles towards Lake Erie, has been supposed to be very remarkable. I maintain that we have more striking examples of erosion than that. This is not the most remarkable example to geologists, though it may be to common observation.

Allow me to state how I suppose that the water acts in wearing away the surface of the earth, the rocks as well as the soil. It operates in two ways; chemically and mechanically. It acts chemically, by dissolving the substances, being almost the realization of the alchemist's dream of a universal solvent. It acts mechanically, if not by its own friction, by carrying along rough substances which wear away the rocks and earth.

Ice acts only mechanically, but in two ways. First, by its momentum, tearing down and wearing away obstacles; and also still more powerfully, by expansion. By flowing, when fluid, into crevices in rocks, and afterwards congealing, it exerts an irresistible erosive power. This year it will enter a small crevice, and expand it very little perhaps; but next year the crevice will be larger, and the power of the ice be increased; and thus the work goes on in a geometrical ratio.

The effects of these various modes of action, of water and of ice,

are seen upon the shore of the ocean, in its wearing away the coast. Who, for instance, that is acquainted with the coast of Massachusetts and Maine, will doubt that Boston Harbor and even Massachusetts Bay have been thus produced mainly by the agency of northeasterly storms. The rocks around Boston are softer than the granite forming Cape Ann and the shore of Cohasset, and therefore, have they been more worn away by the unceasing action of the waves; although the drift agency doubtless aided in this work.

There is another curious class of facts in our country. There are singular gorges made through some of our rocks, to a distance of even half a mile in some cases, and to a depth of 70 or 80 feet, where it is obvious that some agency or other has removed the rock 60 feet in width. Yet there is no stream there. It appears like the bed of an ancient river, though sometimes on the top of a hill. These are called, for what reason I know not, Purgatories. There is one in Sutton, some three or four hundred feet above the ocean, and half a mile in length. The width is 70 or 80 feet; and the depth, 50 or 60 feet. There is another in Great Barrington, Mass., I should think 700 or 800 feet above the ocean; where there is a whole hill cut directly across by one of these gorges. Now it might be difficult to find out the cause of this, were not the same work going on at present in other places. We have only to go to Rhode Island to see how it is done; for there are two purgatories now being formed. A little east of Newport, you will find the conglomerate rock having a jointed structure. There are fissures running parallel to one another, and across the strike of the rock. Suppose two fissures eight feet apart, and upon the coast. The rock between them is broken out by the freezing of the water which flows into the cracks; and every year the work goes on farther and farther. This is just what the water is doing in Rhode Island. It has split out eight feet of rock in width, and left walls 70 feet high, and has thus found its way inward seven or eight rods; and the waves still rush into the end of it. There is another one to the south of Newport, where the water has forced itself in the same way into granite, and hence has not produced so much erosion.

Now suppose such a work as this to have been going on among the rocks of Sutton and Great Barrington, and you have an idea of the manner in which the ocean operated when these spots were just upon the shores of the ocean.

I said that there was some evidence that the ocean was formerly above this continent. I think these gorges are proof of that fact;

though it requires the truth of this explanation to be admitted. But I know of no other hypothesis which can be suggested to explain the facts, and I have no doubt that this is the way the purgatories were formed.

I shall now lay down a series of propositions, which I think can be proved, although I am not prepared to bring forward all the evidence which has suggested itself to me. And moreover, I have as yet by no means completed my examinations.

First; the older the rock, over which the river flows, the greater may we expect the erosion to be, because it commenced earlier.

Secondly; where similar rocks are eroded by rivers, we should expect nearly the same amount of excavation. This is a point of great importance, and, I think, one which has not received the attention of geologists. It must be remembered, however, that the newer rocks are softer than the older ones; and allowance must be made for this difference. I will quote a few examples to illustrate these points.

I have alluded to the cutting of the Niagara River through the Silurian rocks of Western New York, for seven miles. That is not, I said, an unique case. Only come this way a little, to Rochester, and on the Genesee River you find another cut of seven miles, through essentially the same kinds of rocks. On the Oak Orchard Creek is still another place, where the fall has gone back about the same distance. These seem to me some proofs of the theory, that in the same kind of rock you may expect the same amount of erosion. I will now allude to two cases of cuts through limestone, though not of my own observation. The vast caverns in our western States, were undoubtedly produced by subterranean rivers wearing away the limestone.

On the eastern continent, in Asia, if you go to the Dead Sea, and then pass south only to the Red Sea, you gradually ascend through the valley for 60 miles. The bed of this valley is soft limestone, and in that limestone there is an excavation, called Wadde el Jeib, that is 100 feet deep at its mouth, half a mile wide, and nearly 40 miles long. There is no water in it, in the summer, but the winter torrents are powerful, and they have worn out that long gulf.

In mica slate and gneis, which are about of equal hardness, I will mention two cases; one of which I have examined this summer, in company with Prof. Henry, Prof. Guyot, Mr. Saxton, and one or two other gentlemen. It is west of Washington, about 16 miles, upon the

Potomac. Their height at Great Falls, above tide water, is 112 feet; the whole fall is 82 feet. Perpendicular walls of hard mica slate, whose strata run in the same direction as the river and dip east about 70°, enclose the river to the height of from 60 to 70 feet, at least four miles, and farther down the banks are high and near, even as far as Georgetown. That river has certainly worn back, under most unfavorable circumstances, at least four miles, to a height of 70 or 80 feet. You can see the walls three or four miles down the river, as you stand on the bank. Now, to my mind, this conveys the impression of a much longer abrasion than that of the Niagara of Western New York.

Another example may be found in Massachusetts, upon the Deerfield River. The river seems to have cut down for seven or eight miles through mica slate and gneiss to the depth of several hundred feet, the strata running across the stream at an angle of about 45°. Directly west of Sherburne Falls is a high mountain across which Deerfield River now passes 600 feet at least below the summit. Now we are certain that this river once run 80 feet above its present bed; for at that height, on the south shore, we find pot holes from four to six feet deep in the rock; and geologists have never found any other agency to produce these, except river action. In examining to see how far up the mountain I could obtain evidence of river action, I was surprised to trace it to the height of 545 feet; so that it has worn down the valley to that depth. Here then, you have an erosion of 545 feet, and perhaps a great deal more, for the top has been all swept off, in the hardest kind of rock. Now is it any more difficult to conceive of the river's wearing through eight miles of the same kind of strata, between this barrier and the place where it debouches from the mountains into the valley of Deerfield?

Let me now draw a few inferences from the facts that have been stated.

1. We have evidence that the river has worn down this barrier of 545 feet, and probably more.

2. We can see in this case the difference between the drift agency and fluviatile action. The first has cut down the northern slopes and the crest of mountains, and rounded them as if a great plane had passed over them; and left the surface covered with striæ, while the minor crevices and sinuosities are not touched generally. The latter has smothered all the inequalities, but has rarely left any striæ.

While making observations at this place it seemed as if I were

acquiring a new pair of eyes for observing the geological phenomena on the earth's surface. And, since that time, I have found them very useful; nor is this the first time that I have found it necessary to be learnt to see.

3. We may also infer that we have in this case the data for determining approximately the amount of erosion of the river bed since the drift period, and thus may fix more definitely the date of that period. Heretofore, even geologists have been hardly able to guess at that epoch; but it seems to me that we have here a key. For there is a terrace of about the same height as the pot holes — which has been cut through to the depth of 80 feet. Now these terraces have certainly all been formed, that is, the materials have been brought in to the valley, and sorted and arranged, since the epoch of the drift; and then cut down to the depth of 80 feet. Consequently, since those materials were deposited, Deerfield River has worn out its bed 80 feet through gneiss; and that is the amount of erosion since the terrace was formed. But we must go still farther back, although with less certainty. We must go back through the time of the deposition of all these materials of the terraces, in order to arrive at the epoch of the drift. With such facts before us, can there be any doubt that the drift period was long before the time when man with the other coeval animals came upon the globe?

It has been supposed by some distinguished geologists that the drift agency did not entirely cease, until after the creation of man, but it seems to me that such facts as I have mentioned, are strong against such an opinion. I do not suppose that the river at Sherburne Falls has worn down its bed scarcely an inch within the memory of the oldest inhabitant; for it is the hardest kind of rock. What then must have been the time requisite to wear it down 80 feet? And then an equal time, perhaps, must be added for the deposition of the materials.

4. We get from this example some idea of the amount of fluviatile erosion previous to the drift period. This will not be simply as 80 to 545, or 1 to 7. For the barrier was once probably a great deal higher than at present; certainly the ridge is almost double that height to the north and the south of that lower portion where we now can trace river action. But the imagination flags in looking back through a period long enough to produce such enormous erosions.

I would add some other interesting inferences; but they are not mature enough to take up the time of the Association.

Remarks followed by Dr. Hare, Prof. Agassiz, Prof. Adams, Prof. Rogers, Prof. Guyot, Rev. P. Lesley, Pres. Hitchcock, Prof. Hall, Dr. Jackson, and Prof. Hubbard.

On the Embryology of Ascidia, and the Characteristics of New Species from the Shores of Massachusetts. By Prof. Louis Agassiz.

I will merely give my remarks in the form of aphorisms, as it is somewhat difficult to explain such phenomena without diagrams, and as I shall have occasion to bring up again this subject when publishing my illustrations of the structure of these animals and their development from cells.

In Ascidia, I have traced not only the changes which the egg undergoes, but the formation of the egg itself, and all its changes up to the time when the forming embryo had undergone all its metamorphoses and assumed its final form. The facts I have observed show distinctly that there are cells developing in the animal kingdom, in a way entirely different from the views generally entertained by physiologists about this process. Without entering into any discussion, I will state what I have seen, and upon facts every one will be able to form his own theories as he chooses.

If we place under a microscope an egg of an Ascidia, in its primitive form, we notice that it is surrounded by membranes which are transparent. The outer one is the vitelline membrane ; the middle one is the germinal vesicle ; and the contents are as yet perfectly transparent. In time there is a change, and instead of simply two such membranes and no granules in them, we observe the germinative dot, and granules within the outer vesicle ; by and by the vitelline mass has become somewhat more condensed, and the few cells, which could be observed in it at an earlier period, have now become much more numerous. The germinative dot, which was at first a simple vesicle with transparent contents, we observe continually growing. We notice in it not only granules in greater number, but these granules have grown into vesicles, and these vesicles are nucleated.

So that we have here evidently the transformation of a primitive cell, having no germinative dot, into an egg in which we have such a vesicle containing nucleated cells, and not simply granules It follows, from these simple facts, that the granules, by swelling, are

transformed into cells, and that the contents themselves are the material from which these cells are gradually developed.

And how the vitelline mass itself is enlarged, I am prepared to state. It is by the same process: the nuclei of the few primitive cells, growing themselves into cells, which burst the membrane of the first developed cells. Therefore by evolution, by the swelling of the granules in which new contents are formed, which themselves are capable of assuming this vesicular form, we have a mode of formation of cells which differs from that which has been observed elsewhere. I would mention that upon this egg of the Ascidia, I have observed all over the vitelline membrane a layer of epithelian cells; so that it would be difficult even for an experienced embryologist to distinguish the egg of a Rabbit from that of an Ascidia, so closely allied are they in their earlier condition.

What renders investigations in the Ascidia so easy and so successful, is the fact that in every ovary we may find eggs in all stages of growth, from eggs just building up to the highly developed new individual; so that every time we place a mass of eggs under the microscope, we have a complete series for comparison before our eyes.

The figures of the contents of these germinative vesicles with nucleated cells, I would like to submit to those interested in the matter, as it is a point which I think has not been observed in embryology, that the *germinative dot* should contain *vesicles* which have themselves a *nucleus*. Now, after these contents of the egg have been worked out, as it were, by this process of the growth of new cells, the whole mass undergoes the common process of division, as it usually takes place in those germs in which the yolk divides completely. The mass is divided, first into two, then into four, then into eight, &c., and the masses thus divided are surrounded by walls as soon as the division takes place. This division is continually repeated, until finally, from this repeated division and subdivision of the yolk, the materials of the contents of the egg are reduced into small particles, each of which is surrounded by a membrane. Here, therefore, another mode of formation of cells is introduced; cells formed around materials preëxisting to their formation; whereas in the first instance we had cells arising from granules which swell and produce new materials within. I know of some other modes of formation of cells, but as they do not belong to this series of observations, I will not remark upon them, but simply insist upon this point, that we have now, besides the facts which have been pre-

sented before, as to other modes of formation of cells, two well distinguishable modes of formation of cells observable in the egg of Ascidia.

How theyoung germ is developed and what changes it undergoes, I will not describe for the present; but simply state that there is a period, after the subdivision of the yolk has been completed, and after these embryonic cells have been formed by membranes surrounding materials preëxisting, when the germ itself has a pear-shaped appearance, and a long tail-like appendage, and eye specs upon its anterior surface. At this period, the germ of an Ascidia resembles those full grown Ascidians which are provided with a tail and are attached to the bottom of the sea, such as Boltenia; showing that the lower forms in all families are really analogous to the embryonic condition of the higher forms belonging to the same family, but which when full grown are thrown off.

After these general remarks, I have nothing more to say, but to mention the species found upon our shores, of which I shall shortly publish figures.

Ascidia amphora, *Ag.*, the most common of all, confounded hitherto with the *Asc. rustica* of Europe.

Ascidia psammophora, *Ag.*, whose body is surrounded all over with fine sand. Found around Cape Cod.

Ascidia ocellata, *Ag.*, a beautiful tubular species, almost transparent, having a circle of red dots (eyes) around the openings. From New Bedford.

Ascidia carnea, *Ag.*, a small species, red colored, with a smooth body. Lives in deep water, upon shells. Not immature.

Ascidia hirsuta, *Ag.*, still smaller, of a rose color, dotted with white; of an intense red around the square apertures: the body is covered with fringes. Off Cape Cod.

Ascidia rugosa, *Ag.* — Surface warty, brown; apertures square, of purple color.

Boltenia microcosmus, *Ag.* — Sac larger and rounder than in the species to which it has been formerly referred. Chelsea Beach and Point Shirley; also George's Banks.

Description of Certain Mineral Localities, chiefly in the northern part of Worcester and Franklin Counties in Massachusetts. By Charles Hartwell and Edward Hitchcock, Jr.

The chief object of this paper was to give an account of a region in the northwest part of Worcester County, and the northeast part of

Franklin County, which has furnished several interesting species of Minerals, and promises to furnish more. The authors first give some account of the geology of the region; then of the species of simple minerals found there, and finally of the crystallography of those species which present rare or peculiar forms. The principal species described are Beryl, (of which the Royalston locality has furnished probably the finest and most abundant specimens of any place in this country); Allanite, Titanic Iron, Feldspar, in crystals of enormous size; Rutile, of which the bare locality has produced some fine crystals; Crystalized Mica; Epidote, very fine; Babingtonite; Henlandite; Bencholzite; Rose Treanto; Cinnamon Stone; Idocrase; Magnetic Oxide of Iron; Schorl, &c. &c. Among the crystals presented was one of Spodumani, (from Norwich, in Massachusetts,) with several secondary terminating planes.

Adjourned.

C. B. ADAMS, *Secretary*.

August 16*th* — *Afternoon*.

GENERAL SESSION.

A COMMUNICATION was presented by Lieut. MAURY, of the National Observatory, upon the Winds and Currents of the Atlantic Ocean.

Prof. BACHE remarked, that the points investigated by Lieut. MAURY were not only interesting to navigators, but also in a scientific point of view, and would undoubtedly lead to most important generalizations in regard to the winds and currents. The fact which Lieut. M. had mentioned, of the curves of the winds towards the continent, seemed at once to point to the Gulf Stream as perhaps the cause of the rarefaction producing those currents. He also alluded to the fact that Lieut. M. had found some regions so little known, as to make it desirable to explore them with more precision than could be done by ordinary observation not especially directed to such an end, both for the sake of ascertaining the direction of the winds and the temperatures. He hoped that the Association would assist in the work, and would lend their influence to Lieut. MAURY to procure the means necessary for the purpose.

Prof. PEIRCE related the anecdote of an old Greek philosopher, who, having been reproached that science was useless because money could not be made out of it, foreseeing that it was to be a very good

season for grapes, bought all the wine-presses, and made his fortune out of it; by that means silencing all cavillers.

In one fact related by Lieut. MAURY, there was an answer to all who doubted the use of scientific investigations upon winds, showing the vast pecuniary benefit to the country which had already resulted from these researches. Four days saved, in all the vessels and all the commerce from the United States to Rio Janeiro, or in that direction, and probably upon all the commerce of Europe in that direction! Who could calculate the annually increasing value of a saving of four days' sail of every ship passing in that direction? In a scientific view, it was easy to see that by means of these charts, new light had been thrown upon the subject of the trades and the monsoons. The whole thing was a most striking proof of what could be done by pursuing things in their right way. A happy method of combining results, was the foundation of this improvement. What a rich field was open for future research! He agreed with Prof. BACHE in the hope that the Association would pass resolutions, or in some other way promote the progress of these investigations, and inquired if a Committee had not been already appointed to request the aid of foreign navigators.

Lieut. MAURY replied that such a Committee had been appointed; but he did not know that they had taken any steps to this end. He supposed that all that was wanted was to ask for the log-books of the myriads of foreign vessels, to obtain them.

This subject was, on motion of Prof. BACHE, after remarks by Prof. PEIRCE, Dr. HARE, Prof. W. R. JOHNSON, and Lieut. MAURY, referred to the Standing Committee.

On motion, Lieut. CHARLES H. DAVIS was added to the Committee upon the Prime Meridian, he having previously declined, but upon further consultation having been induced to accept.

Prof. HENRY then presented an account of the photographic registering magnetic apparatus, used at the observatory at Toronto.

After some remarks by Dr. HARE, the Association adjourned to meet at half-past 7 o'clock in the evening, at Lyceum Hall.

E. N. HORSFORD, *Secretary.*

Third Day, August 16, 1849.

EVENING SESSION.

The Association met at 7½ o'clock, P. M., in Lyceum Hall.

The following oral communication was presented: —

On the Progress of the Survey of the Coast of the United States. By Prof. A. D. Bache.

In saying a few words this evening to the Association on the subject of the past and future progress of the Survey of the Coast of the United States, I need hardly speak of the importance of the work — of its importance as a correlative to navigation, of its importance in view of the great commercial interests of every part of our country. Opinions are not divided upon that subject. Nor need I trouble you with statistical details; but rather, addressing to you a plain statement of the progress of the work, by the aid of diagrams, which will address the eye, let me endeavor to show what has been done towards the Survey of the Coast in times past, what is now being done, and what may be the future progress of the work until it is finally brought to an entire completion.

The Survey of the Coast was first proposed by Mr. Jefferson, in 1807. Congress acted upon the subject but tardily. The Executive was even more tardy after the law had been passed. At that time the importance of the work was hardly understood. The work was greatly in advance of the times. Mr. Gallatin sketched the plan of a magnificent geodetic work; one, which, embracing the Survey of the Coast of the United States, should connect with it a survey on the water of all the approaches to that coast. It was no small work. It was not limited to the survey of harbors, nor to the small extent of country capable of being grasped by the ordinary modes of surveying; but it was a geodetic work in which astronomical observations should fix the principal headlands of the coast; in which suitable bases should be measured for triangulations, and the distance between points should be accurately determined by reference to those bases; in which the topographical features of the coast, as far as required by the navigator, should be ascertained; and in which the hydrography of the coast, connected with this great geodetic operation, should be made perfect and complete in all its parts.

Mr. Hassler, a gentleman from that nest of republics and republicans from which it seems we still love to draw for the science of our

country, — Switzerland, — Mr. Hassler was placed in charge of the work. He was a man, like the work itself, in advance of the time; he was one of those far-reaching men who find nothing around them ripe for action, and who must be content to bide the slow progress of circumstances before they can be perfectly understood. He found no instruments in this country, nor indeed in Europe, at that time, which came up to his idea of what was required in a great work such as he figured to himself should be the Survey of the Coast of the United States. He therefore obtained permission to go to Europe, and, from drawings of his own, had the instruments made with which the survey was to be commenced.

We all of us know how impatient our countrymen are with regard to all such matters. Now here was a great work to be commenced in a country which was not ripe for it. Mr. Hassler, no doubt, had a clear idea of all the steps of his process; but where were the men to assist him? Where were the men to understand him? Where were the men to lend him sympathy and support in such an immense undertaking? He must necessarily have gone slowly for the want of agents to assist him, of persons trained to this kind of work. In 1816, Mr. Hassler commenced the survey; and in 1818, from the idea that he was proceeding too slowly in the work, it was stopped. Just about the time that he was in his prime, the government stopped the work; at least as far as his direction of it was concerned.

It was revived in 1832. The sixteen years which had elapsed might have been time enough to give his plans their full development, and perhaps to finish the coast, as it stood then, and when he was in his prime. When he resumed the work, it was at an advanced period of his life; but still, with that indomitable perseverance and zeal which characterized him, he determined to carry out his original plan. When the Survey of the Coast was renewed, in consequence of the law of 1832, he seemed indeed to have more extended ideas of what the work might be made. We may judge of this, from the fact that the second series of operations which he undertook, was upon a larger scale than the first, and more in accordance with the geodesy of modern times than those with which he originally commenced. His instruments for measuring horizontal angles, made at that day, under his direction, remain now, as good, if not better than any which have been used in the surveys abroad. In 1844, when he was rapidly developing his ideas, because he had formed assistants to aid him, because he had efficient officers of the army, officers of the navy, and effi-

cient civilians to coöperate in the work, trained to its duties, he was taken from his adopted country, and his labors were closed by death.

The first year with me, the year 1844, was, so to speak, a year of observation. I had heard, of course, as you all had heard, complaints of the slowness with which the Coast Survey was going on, from those who never inquired, perhaps, as to where the blame lay, or where the remedy lay. I saw that the survey was proceeding steadily. I saw that we were sometimes made responsible for that over which we had little or no control. In pecuniary matters, when one has a certain sum assigned him, to do a certain work, and when he has done all that sum will enable him to do, how can you say to him, "You do not go on fast enough;" unless he should show his inability to use more means when he asked for them, and they were poured upon him? I am not aware that the means placed at the disposal of any superintendent of the Coast Survey have been too large, nor do I know that any superintendent has been behindhand in asking from the Executive and from Congress, the means which he deemed necessary for the work. It must be obvious that, when it is a question of time, if you increase the expenditure, the work will go on in proportion. But if it is not a question of time, but a question of what is called an economical administration of the government, then give us time. Give us one or the other.

Still it appeared to me that, in some way or other, the work might be accelerated. Feeling that same nervous impatience which belongs, I suppose, to all Americans, I felt as if the work might go faster; as if the experiment were worth making, at all events, if I could get the means to make it. But then I had all Mr. Hassler's foundation to build upon. It was not the foundation I was laying; but I was building part of a superstructure. Whatever I might originate in the work, it could be only a new form of superstructure. But there were the persons ready trained; there was the country greatly advanced. The colleges and other institutions of the country had been pouring out educated young men. In West Point they had been teaching these very branches of practical and theoretical science, and naval officers had been students. The science of the country was altogether upon a different level in 1845 from what it was when the survey was proposed in 1807, or when it was commenced in 1816. So that it was only necessary to find some way of accelerating the work, in accordance with legitimate scientific principles, and to obtain the means, for the men were not wanting.

Mr. Hassler's plan was to commence the work at the great commercial mart of New York, and then extend it in a direction southerly and northerly from that; working at the two ends. I saw that if we were only to extend it at the two ends, there was a limit put at once to progress. I saw that it was necessary either to be satisfied with that progress, by extension at the two ends of the work, or to adopt a system of separate centres, where the extension of the work would depend entirely upon the means at the disposal of the superintendent; because, having these separate centres, he could indefinitely multiply the number of agents to whom the work might be entrusted; and the number would, therefore, be limited only by the means placed at his disposal.

The question of the manner of making the division depended upon the then progress of the work itself, and the manner of operation. The plan of operations in the Coast Survey is briefly this: —

First, a base line is measured by the most accurate means we can devise; say a line of from six to ten miles in length. We have some bases as small as six miles; and we have one of upwards of ten miles in length.

Having established the length of this base, a series of triangles is adopted, with gradually increasing sides, the lengths of which, or the distances between the angular points, are known from the base and measured angles. By measuring the angles from the extremities of the base, the length of which is exactly known, we obtain the direction and distance of a new point, and thus a new base line. That line, in turn, establishes another at a still greater distance; and thus the surface of the earth is covered with a net work of triangles, each side of each triangle being calculable from the measure of the angles, and from the length of the originally determined base. This would be easy, were the triangulation upon a plane surface; but the calculation is not so simple as it would at first appear. The earth must be considered, in computing these large triangles, not merely as a sphere, but as a spheroid. But I will not enter upon that, at the present time. This triangulation has the advantage, you will observe, that points, between which you cannot measure, may be determined with perfect accuracy, within any assignable limits you choose to impose upon yourself.

Next comes the astronomical part of the survey. The direction of the lines with reference to the meridian must be known. The latitude of the points must be ascertained, and their longitude must be

determined. Having these data, we then know from computation the latitude and the longitude of any point, and the angle with the meridian of any line connected with the triangulation. This is the second part of the work.

We have thus a great scheme in which the progress may be very rapid, because the steps may be very great. You may have, — as we have in some cases in our New England triangles, — a side of 60, 90, or even 100 miles in extent; and thus you may make vast strides along the coast at once, by taking advantage of hills properly situated in the interior of the country and overlooking the ocean shore.

Having determined these points with great nicety, with large instruments, and much care, the work between them need not be done so elaborately. New points are determined between the former upon the same great system of triangulations, called the secondary triangulations. Upon this, with a still less exact mode of working — namely, with the plane table — the topography is laid down so far as it may be necessary, to show the coast to the navigator, and for purposes of defence. All the points are checked by the secondary triangulations, which in their turn are checked by the primary; so that having taken great pains in the first part of the work, you cannot wander far out of the way in the second part of the work, or in the topography.

Having thus determined the outline of the coast, the hydrography gives you a picture of the bottom of the sea, just such as the topography gives you of the land above its surface; and this completes the survey.

Now these triangulations, the astronomical measurements, and the topography, had been carried, at the time that I took charge of the survey, into Narragansett Bay, on the one side, and into the Chesapeake Bay upon the other. It was a plain thing, then, for me to go on with the triangulation to the eastward, and to carry on the work to the southward, thus burning the candle, as it were, at both ends. I preferred, however, to divide it into parts and set other portions to burning also. Estimating, as well as I could, from the best means to which we had access, the shore line of the coast — by which I mean to include all the indentations, estuaries, rivers up to the head of tide water, and harbors — I endeavored to divide the coast into as many sections as I thought the means I should be likely to obtain would allow me to commence the survey in.

The question was asked me about this time, how long I thought the

survey would be in finishing. This was at the session at which Texas was annexed; and I asked the gentleman what extent of coast he meant to include? Did he mean to the St. Mary's? Did he mean to include Florida and Louisiana, which had been purchased after the survey had been commenced? Did he mean to include this coast of Texas, which we had just been adding to the Union, and which alone had added two years to the duration of the survey? Since then Oregon has been made a territory, and California acquired, and thus the limits of our coast have been greatly extended, and with this extension the importance of the survey has greatly increased.

Originally, to return to my statement, I divided the survey into eight sections. Texas made a ninth section. Oregon makes a tenth. Now I will show you, as well as our time will permit, how far we have progressed in the several sections; I have had these diagrams colored, so that you can almost see for yourselves the whole extent of the work.

[Prof. B. then proceeded to explain from the diagrams the progress of the work.]

In the first section, from Point Judith to the boundary, we have now finished the primary triangulations, from the base on the Boston and Providence Railroad, along the coast of Rhode Island, Massachusetts, New Hampshire, into Maine. The point which I expect in a few days to occupy, is in the neighborhood of Portland. Now it would be very easy for me to calculate how long it would take me to extend these primary triangulations to the boundary; because I have the statistics of this part of the work; I know how long it has taken me to do a similar amount of this work. But my object is not to carry the operations at once to the boundary, in the shortest space of time, because I am already in advance of the secondary triangulations. This is a part of the coast where the primary triangulation may be carried on rapidly. The hills of New England, as Mr. Hassler said, seem as if they had been made for triangulation. They are so situated that we can pass rapidly along the coast with long strides; and the only difficulty is to avoid being tempted to make the lines too long. I have frequently, however, in taking such long lines, taken also an intermediate shorter line, for the purpose of verification. We have then passed, in five years, on the primary triangulation, from Rhode Island to Maine; and it could have been done in three years, had it been desirable so to occupy the time. But being so far in advance, I am enabled to occupy a portion of the season at the

South, in the Autumn and in the Spring, and thus not to confine the field of my personal labors to one section, but to spread it over many sections, and particularly to establish the bases upon which the triangulations in the different sections rest.

The secondary triangulation has extended along the shore, determining the positions of points near the coast, around the peninsula of Cape Cod to Cape Ann. There are now two parties at work upon this step in the process; one passing from Cape Ann to Newburyport and the other from Newburyport to Portsmouth; so that by the end of the season, we shall have reached nearly to Saco, in Maine, with our secondary triangulation.

The topography has been carried regularly forward in the same way, with but one exception, in which I have perhaps taken some responsibility. I certainly did have Boston Harbor surveyed two years before it would have been done in the regular course. But then I had a very violent motive for this; namely, an appropriation made by the State of Massachusetts to hasten the survey of the coast; an act of liberality which has never been imitated by any other State in the Union. We have two large manuscript maps of Boston Harbor, which you will see in the State House, in the month of October next, of a very finished character.

Thus, in six years, (adding the present year,) at this end of the work, we have advanced from Point Judith, with the primary triangulation, to Portland, with the secondary beyond Portsmouth, and with the topography to Gloucester.

The hydrographers have had a long and difficult piece of work in this section. They have had those famous Nantucket Shoals to stop them. But if the Survey had rendered no other benefit to the country than making known the before unknown and hidden dangers of that part of the coast — dangers, because unknown and hidden — it certainly would have repaid to the country in money the whole amount which it has cost. One vessel which came very near stranding upon shoals — now, through the enterprise of Lieut. Com'g Charles H. Davis, made known and familiar to us — had a cargo which paid the Government a duty of $125,000; and if this sum had been devoted to the survey, the shoal would have been discovered years ago. I have endeavored to mark upon this map the discoveries which Lieut. Davis has made. They consist of an important shoal outside of the Old South Shoal of Nantucket, lying directly in the track of vessels from New York to Europe and returning, and of vessels

passing from the New England States to the Southern States, and South America. The dangers which he has developed, and six of which, he made known last year, have, for all time, enrolled his name among the benefactors of his race.

The hydrography has been extended up Buzzard's Bay, through Nantucket Sound, and through the Vineyard Sound; it has embraced the Nantucket Shoals, and has included the hydrography of Boston Harbor, an accurate chart of which has been made.

The results of the survey pass through a regular process, from the time the observations are made in the field, to the time the map is produced in the office. The assistants, who make the observations, report them and compute them. Other computers also pass over the same calculations. The results are brought into juxtaposition and compared. If they agree, they are considered as correct. If they disagree, the cause is carefully examined and the error corrected. The results, thus verified, are placed upon paper in the ordinary forms of projection of maps. They are next engraved, as fast as we can find hands to engrave them; and when engraved they are made public.

We have published, within the five years past, twenty-five sheets of maps of a very finished kind. They have been examined, by our own citizens, and by foreigners; and I believe with approval in every case. I have carefully compared them with foreign maps, in order to see where we stood, and what we had to learn. The arrangements for this part of the work are not now quite adequate to the demand of the field work. The force of computers requires to be increased; the force of draughtsmen and of engravers requires to be increased. It is a remarkable fact, such is the prosperity of this branch of art, and such the demand for engraving in common life, that it is almost impossible to get a good map-engraver to leave his home for any inducement I can offer him, to come to Washington and place himself there under our direction in the office. Our attempts to procure work by contract, out of doors, in a finished style and with exactitude, have been, in some instances, partially successful. It was a source of complaint that Mr. Hassler sent abroad for engravers. Now I know that they cannot be had at home. When we wanted to increase the force of our engravers, we could not tempt, by any reasonable emolument, such engravers as we wanted to engage in the service of the government. The reply was a natural one, and one which I could not meet by argument:—"I am

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at home here; my family relations are all here; I do not wish to change them; I have as much to do as I can attend to; and I can earn just as much as I need, and therefore do not wish to leave my home nor to make any engagement with the government."

It is easy to see when the first section of the work may be finished. There are about sixteen stations to the boundary, which could be occupied in two years and a half, at the present rate, making the astronomical observations as well as the geodetic. I do not propose to do so, because my time in the spring and autumn is better employed in other positions; and it would be better, therefore, to occupy two stations in the north in a year, than to occupy six or eight, as I have done when it was necessary, in order to get ahead of the other operations of the survey. So much for the first section.

The second section is done, excepting the work of verification and making necessary changes. There was a rich harvest of hydrography in Long Island Sound, — discoveries of detached rocks, about which little had been said. But in the case of the entrance to New York harbor, there was a richer harvest still; for there Captain Gedney found a new channel, now called by his name. This was either a new channel, or a channel which had long existed, but was newly discovered, most probably the latter, and that in the progress of the hydrography of the Coast Survey. The advantages of a channel, having two feet more of water in it than the main ship channel, will be appreciated by all. Buoys have been placed in it, and it is easy to find the way out and in.

It was reported last year that it was filling up; which, in passing, be it said, is not true. It is often remarked that the coast is changing every year, and that there is, therefore, little use in surveying it. The truth is, that there are a few points in which the coast is really changing, and those points should be carefully watched. We should know where they are, and why they are changing; how to stop the changes, if it is necessary, and how to avail ourselves of them, if it is necessary. But in New York harbor, it was the easiest thing in the world, at a trifling cost, to have the hydrography repeated, and the result showed that there are not six inches of water, more or less, than there were when the survey was made, so that the changes which have taken place in the harbor, if any, are exceedingly slight. And, considering the nature of the operations of sounding, I should say that there had been probably no change. The discovery remains valid to this day — the goodness of the channel unimpaired.

The discovery or determination of three channels in Delaware Bay, rewarded the exertions of the officers engaged there — Captain Gedney and Lieut. Davis. These channels are not of so much interest as the channel into New York harbor; but they are of very great importance. One of them is now constantly used by vessels carrying coal from Philadelphia to the eastern parts of the Union; buoys having been placed in it, so that it is known. Another one enables vessels to pass directly across to the breakwater, when they are lying in Cape May Roads, and the wind comes out at northwest, exposing them to the dangers of a lee-shore.

It has been asserted also of Delaware Bay, that it is constantly changing. I have had the entrance resurveyed within two years, and the result shows that the changes of the entrance have been slight indeed. No change that would be perceived in an ordinary survey, has been indicated since the survey six years ago.

This section of the work (the second,) then, I consider as completed, excepting a portion of the work of verification. The off-shore hydrography along the coast is better described by the depth of water, than by the distance from the shore to which it has been extended. The depth to which the soundings are carried, was one hundred and twenty fathoms. The off-shore work from Black Island to Cape Henlopen has been represented upon a chart, now in the course of engraving, which I presume will be completed by next February.

For reasons which I have endeavored to intimate this evening, although I have not fully developed them, Mr. Hassler did not immediately publish his results; and in taking charge of the survey, I found a large amount of back-work to be brought up. The idea I have had, from the first, has been to bring up the back-work, and then to keep the publication abreast of the work itself; and I have nearly been able to effect this, though the work has been increased rather faster than the means for engraving have enabled us to publish. The third section extends from the Capes of the Delaware to the Capes of the Chesapeake, and includes the States of Maryland, Virginia, and a part of Delaware. The primary triangulation has been carried down the Chesapeake into Virginia, and I have very little doubt from the report of the assistant who had charge of that part of the work, that the triangulations will, this season, reach the James River. The secondary triangulation already extends, both in the Chesapeake Bay and on the ocean shores, into Virginia.

The connection of the Capitol and National Observatory with the

coast stations, has been nearly completed. Now it is plain that we may estimate when this section will be done. The primary triangulation cannot occupy more than two years; the secondary, perhaps a year more; the topography, which has so far kept pace with the triangulation, we may suppose will be a year and a half or two years behind, so that the section must be finished in four or five years. Then there is the hydrography, which has been brought down from the Capes of Delaware Bay very nearly to the coast of Virginia, and down the Chesapeake into Virginia, so that we expect, before the winter season, to reach the mouth of the Rappahannock River in the Chesapeake, and outside, to reach the southern part of Broadwater Bay. There is one operation of which I did not speak, because it was so familiar to everybody — the reconnoissance or preliminary examination made so as to know exactly where the triangulation is to go. In this section the reconnoissance is completed, as in the preceding sections to the eastward.

The fourth section embraces the State of North Carolina. The primary triangulation has been carried from the base measured on Bodies Island, up the Albemarle Sound, into Pamlico Sound and the sheet of water, which under the various names of Roanoake Sound, Currituck Sound, &c., fills the space between the sandy part of the coast of North Carolina and the swamp which immediately joins it — these Sounds constituting the great line of communication between the North and the South, through the Dismal Swamp Canal, from the Chesapeake Bay into Albemarle Sound. One advantage derived from the measurement of a number of bases is, that thus we are enabled at once to survey the more important points of the coast. Thus, in this section, we readily reach Hatteras, one of the most dangerous points on our coast. The triangulation which will be carried on this season will include Cape Hatteras, and Hatteras Inlet, a new cove which has already been examined by the Coast Survey, having been previously known only to the pilots, but affording actually a harbor of refuge where it is so much wanted, on this very dangerous coast. Lieut. Com'g Maffit has examined this, in the way of reconnoissance, and a preliminary chart has been published, showing how vessels may take advantage of this refuge.

It is very remarkable that natural causes should tend to counteract natural difficulties. Such a change in the direction of the currents has taken place near Cape Hatteras since 1846, that the point of the Cape has begun to make out, and thus affords a natural protection

behind which there is a beautiful cove, easy of entrance, with a capital anchorage, and perfectly protected from the sea in a northeast wind. Hatteras Inlet, itself, though caused (in 1846) by what the inhabitants considered at the time a disastrous hurricane, affords a beautiful harbor of refuge to the extensive coasting trade passing from the South to the North, and from the North to the South, in the United States.

Now we can only approximately estimate when this section will be done. The reconnoissance was only made in 1843, and the triangulation commenced in 1845. But the period of survey, from the beginning to the end, cannot exceed twelve years, and three of these have passed.

The next section is a very interesting one, comprising the coast of South Carolina and Georgia; interesting, among other reasons, because it has been said that triangulation is there impossible. It is a curious, but a uniform fact, that the coast of the United States, in general, lends itself to this kind of work. Where it is not made up of the bluffs which we have in New England, with the noble hills in the interior, there is generally a sandy island, or a continuous beach of sand, or a hillock, or a piece of morass, within which there is very deep water, forming an internal navigation, and across which the lines are readily run, affording an opportunity exactly for this work. What could be better than Albemarle and Pamlico Sound on the flat coast of North Carolina. In South Carolina, the difficulties seem to accumulate; the wide bays, separated from the ocean by a narrow strip of land, disappear; but, nevertheless, from Bull's Bay to the St. Mary's, a scheme of triangles has been found perfectly practicable, and a part of them have actually been traced upon the surface of the ground itself. The base was selected upon Edisto Island, and these triangles upon the map show the manner in which the work is to advance in the easterly direction. The triangulation and topography of Charleston harbor are finished; and the astronomical determinations made. That being an extremely important part of the section, we have made great efforts with regard to it, and the hydrography of the approaches has been actually commenced.

We have examined the whole of this section with care. We have commenced upon an important part of it, and by no means the easiest part, and have met with fewer obstacles than might be expected. We have completed the land part of the survey of Charleston harbor, and I suppose that by next season the hydrography will be added to

it; and as soon as that is finished, the map will be put into the hands of the engraver.

We have not materials for computing the date when this section will be finished, but can give a very fair estimate of it from what we know by the reconnoissance, and limit the time for the land operations to two or three years. I have not supposed that we could undertake more than six sections out of the eight unfinished on the Atlantic and Gulf of Mexico. When any one was entirely finished, I could take up a new section. Last year, by the wise liberality of Congress, an additional appropriation was given for the Florida coast, which has enabled me to begin another section. This is that important part of the coast embracing what is called the Florida Reef and Keys, and consisting of the dangerous reef and of the chain of islands running westward from Key Biscayne, and ending in the Tortugas. The reconnoissance of this chain of islands has been nearly completed. A preliminary base has been selected, and the triangulation planned, which will be commenced in the autumn. In the mean time, as Key West affords one of the best harbors upon the coast, and has been admirably surveyed by the Topographical Engineers, we have connected the triangulation for the survey of the coast with the local survey, and shall add so much hydrography as is necessary to give the deep soundings and the approaches, with some other particulars not included in their object, which was chiefly for defence, and thus avail ourselves of their labors to produce at once a map of Key West and its approaches. Lieut. Com'g Rodgers has, with zeal and enterprise, gone into this work, and I hope from him the completion of the hydrography of Key West and the reconnoissance of Bahia Honda. The astronomical observations required at Key West have been already made, and as soon as the autumn permits, the work in this section will be vigorously prosecuted.

The reconnoissance of this part of the coast which has been made, shows that it is admirably adapted to our methods, affording points at convenient distances and in suitable positions.

In section seventh, the reconnoissance has been made from Mobile Bay towards the east, to include Pensacola Bay, connecting the work with the base line measured on Dauphin Island, in Alabama. This section will exemplify the mode of working by frequent bases and a comparatively small triangulation. The other work, however, from its great importance, has taken precedence of this.

Section eighth is an interesting one, including as it does the high-

way between two most important ports in the Southern country — Mobile and New Orleans. It includes the coast of Alabama, Mississippi, and part of Louisiana. Here the progress of the work has been very considerable. It was commenced in 1845 by a reconnoissance, and followed up, the next year, by triangulation, the measurement of a base and the astronomical observations connecting the stations. Now we have completed the triangulation, from Mobile as far as Lake Borgne, east of New Orleans. Another year we shall finish the triangulation to New Orleans. The topography has kept pace with the triangulations; and the hydrography, also, has very nearly kept pace with them.

Here we have not been any more than in the other sections, without our reward in usefulness, as the authorities of Mobile have testified. The bar of Mobile Bay is actually deepening, and vessels can now carry twenty-one feet of water over it into the bay. As a result of this discovery, the British steamers now come into Mobile Bay, and have even found more water than we had marked, by coming in at a time when the wind had heaped up the water in the bay.

We are sure of the practicability of extending our triangulations with no great difficulty, in this section. The islands, you perceive, lend themselves entirely to the work. We do not, however, know enough of the coast, beyond the Chandeleur Islands, to form an estimate of the time required to complete this section.

Section ninth, including Texas, has been generally examined from one end to the other, and more particularly near Galveston. The primary triangulations have been commenced in the neighborhood of Galveston, and the secondary triangulation is nearly or quite completed in Galveston and Anahuac Bays. The topography will be commenced in the autumn, and the hydrography will at once follow upon that. The astronomical observations necessary have been made.

What progress the Oregon and California sections may show, we can only tell when the California gold mines are a little exhausted. Two parties have been sent to Oregon Territory.

As the triangulations on the Atlantic and Gulf coasts are extended from their bases, they will meet, forming a connected geodetic work; and the bases which now serve as the groundwork of the operations will serve as bases of verification; the work in each section, meanwhile, being adequate to furnishing preliminary maps and charts for the use of navigators.

Connected with this work has been a most important exploration of the Gulf Stream off our coast, from Cape Cod to Cape Hatteras. Of this I shall have an opportunity to speak at another time; and I fear I have already detained the Association too long.

One further point upon which I would remark has reference to some improvements which have been introduced into the work. Some of these are rather of a technical and scientific character, but others are more general.

A gentleman, in speaking of the change in the prime meridian, said to me yesterday, "This will destroy all your coast-survey maps." But we took the meridian of New York as a temporary one; it was avowedly temporary. The meridian of Greenwich is also marked upon our maps. But it is a matter of very little consequence, indeed, to us, whether the meridians have to be changed or not. After the map has been engraved, the next step is to copy it by the electrotype process. A plate in relief is deposited upon the original, and upon this plate, thus produced, we can make any alterations we please. With a common scraper, we can scrape out these meridians; electrotype that again, and we can put in upon the new sheet any new lines we please. We do not use the original plates in printing our maps, because they so soon deteriorate and wear out, so that very few impressions could be taken. We use, in general, the electrotype copies.

I came to the meeting of the Association with some uncomfortable feelings, that Lieut. Davis, who has been so long and so usefully associated with me upon the work, was to be removed from it, even for the purpose of taking charge of the Nautical Almanac. But, as if his mantle had fallen upon his successor, I have learned since I have been here, that Lieut. McBlair, who has taken charge of his party, has continued his discoveries, by finding in the channel out of which the whaling vessels of Nantucket pass to sea, and through which all the coasters bound from New England to New York, and many of those which are bound to and from the Southern States, wend their way, twelve feet of water just in the channel where the charts had nothing less than thirty! Notice will at once be given to warn the coasters that they must steer clear of McBlair's twelve feet shoal, and they now steer clear of Davis's new south shoal.

Pres. EVERETT. At this late hour of the evening, it is impossible to enter into a discussion at all commensurate with the importance of this communication. The audience are agreed, I am sure, that per-

haps no subject has been brought before the meetings of the Association having more important bearings than that now before us. In fact, it is intimately connected with the whole commercial marine, the whole navigation of the United States of America, now the second, and destined to be, perhaps, in no very long period, the first commercial power upon the surface of the globe.

There is one topic, however, to which Prof. Bache, from motives of delicacy, did not think proper to allude ; and that is, the opposition to this great work, by some men of eminence and influence, partly on the ground of its expense.

I remember, Sir, when I was abroad a few years ago, hearing a somewhat amusing anecdote in reference to the curiosity of the Chinese as to the various arts and discoveries of Western Europe, and of the United States of America. Among the things of which they had heard, and of which they desired to learn more, was Mr. Babbage's calculating machine. This fact was mentioned to Mr. Babbage as a piece of information of some interest, viz., that the attention of the Chinese had been drawn to the calculating machine, (as it is commonly called) and it was said that there were few things about which the Chinese were more curious than they were to know whether one of Mr. Babbage's calculating machines could not be made so small that it might be carried in the pocket. Mr. Babbage good-humoredly remarked, upon hearing this, that he had found it a very much "out-of-pocket" concern, and he was afraid that the Chinese would find it so too.

Now, I would not intimate that the Coast Survey is, by any means, an "out-of-pocket" concern, in any proper sense of that phrase; but it must be admitted by all that it is necessarily an expensive one. Well, Sir, Congress are very properly impatient of great expense, and unwilling to grant outlays of money, and justly so, there is so much jobbing constantly going on at the seat of government, and so many chimerical projects are constantly brought forward. Some of the members of Congress, therefore, have looked upon this project with a considerable degree of jealousy, and have thrown out doubts as to its utility. But, Sir, it is quite obvious to every one present, that this is, after all, a prejudice. The Coast Survey, as explained to us this evening, is assuredly a great national undertaking, very expensive, no doubt, but very useful, very important, and, indeed, I may say, indispensable to the prosperity and to the character of the country, as a great maritime State.

Such being the case, I think that it is highly desirable that the force of enlightened public opinion should be brought to bear upon this question, and that it may become strongly favorable; and for that purpose I have arisen to submit to you a motion that the communication of Professor Bache be referred to a committee of this body, to express an opinion on the importance of the work, and to report what course it may be proper to pursue in order to further the prosecution of the work.

This motion was agreed to.

As it was now late, Prof. AGASSIZ having signified his assent, the reception of the communication from him was postponed until Monday evening next.

Adjourned.

E. N. HORSFORD, *Secretary.*

Fourth Day, August 17, 1849.

GENERAL SESSION.

THE Association met, pursuant to adjournment, in Harvard Hall, at 10 o'clock, A. M. After the reading of the minutes of the last meeting of the Association by the Secretary, and the proceedings of the Section on Physics and Chemistry, by Dr. B. A. GOULD, Jr., it was moved and carried that the reading of the proceedings of the Section on Natural History and Geology be dispensed with.

The following persons, nominated by the Standing Committee, were elected members of the Association: — Dr. A. B. CLEAVELAND, Prof. JOSEPH TORREY, JOHN B. FELTON.

Resolution of Hon. EDWARD EVERETT, relative to the United States Coast Survey, was considered. Referred to a committee consisting of Hon. EDWARD EVERETT, Prof. PEIRCE, and Prof. CASWELL, to prepare a memorial to the General Government on this subject.

The Standing Committee reported the assessment for this year to be $1.00.

Also, that invitations to the Association had been received, to hold its next annual meeting at Washington, Charleston, and New Haven, respectively, and that after deliberation it had decided to accept the invitation of the Faculty and Corporation of Yale College, to meet in New Haven on the 19th day of August, 1850.

Also, the nomination of the following list of officers for the coming year: — Prof. A. D. BACHE, President; Dr. ALFRED L. ELWYN, Treasurer; Mr. E. C. HERRICK, Secretary.

It was suggested by Prof. PEIRCE that the Secretaryship of the Society should be a permanent station, as contributing to the usefulness and influence of the Association. Prof. ROGERS followed in support of the suggestion, and thought that some other changes in the constitution might be made with advantage.

In regard to this subject, the President remarked, that any proposition for changes in the constitution of the Association must necessarily lie over until the ensuing year.

Prof. H. D. ROGERS, in behalf of the Standing Committee, offered the following resolution:

Resolved, That a committee of five be appointed to consider the expediency of adding a by-law to the Constitution, authorizing the creation of a select number of honorary members.

The motion was seconded by Prof. B. SILLIMAN, Jr., and unanimously passed.

The following gentlemen proposed by the Chair, for this committee, were unanimously elected: — Professors HENRY D. ROGERS, BENJAMIN PEIRCE, WALTER R. JOHNSON, JAMES HALL, and B. SILLIMAN, Jr.

Dr. C. T. JACKSON presented a communication from a Committee of the Academy of Sciences of France, addressed to himself as former President of the Association. The object of the communication was to provide for the erection of a bronze statue of the eminent Naturalist of France, M. ETIENNE GEOFFREY ST. HILAIRE. Dr. JACKSON earnestly advocated the project; and after debate the matter was referred to the Standing Committee.

The programme of the day having been read, the Association adjourned to meet in Sections.

E. N. HORSFORD, *Secretary.*

Fourth Day, August 17, 1849.

SECTION OF GENERAL PHYSICS, &c.

THE Section was called to order by Prof. BACHE, the Chairman of yesterday, and upon his nomination, Lieut. MAURY, of the Washington Observatory, was called to the chair.

The first paper was by B. A. GOULD, Jr., on the second recorded Comet of 1784.

The Chevalier D'Angos, who was Director of the Observatory of the Knights of Malta, announced in the spring of 1784 that he had discovered a comet. He communicated some observations to the French Academy; and subsequently sent a fuller series to Hindenburg and Bernoulli. (*Leipziger Magazin*, 1786, p. 132.) This series extends over a period of twenty-one days — from April 10 to May 1; but the comet was seen at no other place. It was, indeed, according to D'Angos, very faint. The orbit was also communicated to the *Leipziger Magazin* by the discoverer, who, although he gave no table of comparison between the ephemeris and the observations, yet stated that the maximum of discordance was but 70″ of arc — a tolerable allowance for the error of observation at that time.

Burckhardt endeavored subsequently to determine the orbit, but found unexpected difficulties. At last, in the year 1820, (*Zach. Corresp. Astronomique IV.*, p. 456,) Encke published an investigation of the observations, all of which he pronounced to be a scandalous fabrication. This view was immediately adopted by Olbers. (*Schumacher's Astr. Abhandl.* p. 50, *Encke's Ed. Olbers's Abhandlung*, p. 215.)

Encke's argument for the impossibility that this comet could actually have existed, is derived from a result of his calculations, that if the orbit satisfy three of the observations, the comet must, at the time of the middle observation, have been nearer the earth than the moon is.

Dr. GOULD alluded to the fact mentioned in the *Theoria Motus*, (§ 142, p. 158,) that where the transcendental equation, from which the comet's place at the middle observation is deduced, has three real positive roots; one corresponds nearly to the place of the earth. The argument of Prof. Encke seemed, therefore, not altogether convincing, and he had subjected all the observations to severe investigation, in the hope of a result which might restore the fair fame of the Maltese observer. He gave the results of his computations, which showed conclusively that the observations could not be approximately represented by any orbit described about our sun, in obedience to the law of gravitation.

Prof. Encke had most ingeniously shown how D'Angos had probably fallen into error in computing fictitious observations from an empirical orbit.

Dr. Gould farther stated his authority for the assertion, that Mr. D'Angos was living at the time when the exposure of his fraud was published by Prof. Encke.

Prof. H. D. Rogers submitted a communication on the Analogy of the Ribbon Structure of Glaciers to the Slaty Cleavage of Rocks. He described the ice of glaciers to consist of thin alternate parallel bands or plates of blue crystal ice and white porous ice, each not more than one-third or one-half of an inch in thickness. These pervade the whole mass of every glacier, and are clearly exposed in the sides of the transverse fissures. Near the sides of the glacier they are almost absolutely parallel with its mountain walls, but they sweep away towards its medial line, and form, like all the other planes which divide the glacier, an innumerable series of loop-like curves. This looped or festoon form is obviously caused in part by the downward movement or flow of the semi-plastic ice, and in part by the influence of the terminal moraine, to induce the same parallelism to itself, which the rocky sides of the glacier produce in the ice near them. The most general fact noticeable in relation to these structural planes, is their approximate parallelism to the walls, the rocky line and terminal moraine confining the whole mass, or, in other words, to the surfaces of higher temperature which enclose the glaciers. An appeal was made to special features in certain glaciers, tending to prove that however the direction of these ribbon lines may alter by irregularities in the onward flow of the glacier, their position near the region of the Nevi, where they originate, is strictly parallel with the surfaces of the warmer mountain sides.

Prof. Rogers exhibited in the next place the existence of a kindred law in the position of the planes of slaty cleavage which traverse nearly all argillaceous and even calcareous strata that have been much altered by a diffused heating action. He showed that whether the formations have been much disturbed or not, the cleavage structure invariably approximates to parallelism with those great planes in the crust which have been the planes of maximum temperature. In all the districts of folded or plicated strata which he has examined, including the long southeastern belt of the Apallachian chain in the United States, and the Rhine country, and the Alps in Europe, he has observed that the cleavage dip is parallel to the average dip of the anticlinal and synclinal planes bisecting the flexures; and he showed that these, being the parts of the bent strata most crushed

and fissured, are the chief channels by which highly heated volcanic products, as steam, gases, and molten rock, have passed out to the surface.

Every plicated belt of strata must, therefore, have consisted, at the time it was being folded and acted on by the subterranean forces, of alternate hotter and colder planes dipping in parallel order, and it is to these thermal planes that we may reasonably ascribe the production of the similarly parallel cleavage induced in the half-baked slaty rocks.

He next drew attention to the fact that a perpendicular slaty cleavage is seen to have been produced in horizontal clay shale, by the intrusion of a vertical dyke of melted trappean matter, the wall of the dyke in this instance exposing to the shale a plane of elevated temperature parallel with which the slaty cleavage was established.

He concluded by shewing that one law of position of the cleavage or ribbon structure connects all the phenomena here described, namely, that in all cases it originates in planes parallel to the surfaces of permanently highest temperature.

Prof. A. D. Bache next gave an account of the mode of exploration of the Gulf Stream and of the ocean on each side of it, adopted by him; made a brief notice of the labors of Lieuts. Commanding Davis, G. M. Bache, S. P. Lee, and R. Bache, to whom the explorations had been entrusted in different years; alluded to the instruments employed for ascertaining temperatures at different depths, and gave a brief summary of the facts developed regarding the cold water inside of the Gulf Stream, in the stream, and beyond it; described the division of the Gulf Stream into two or more branches, and the probable cutting off of the cold water inside at Cape Hatteras.

Lieut. Maury followed with some remarks on the curious fact, that, although we were aware of so many currents pouring into the Atlantic, so little was known of any outlets.

Mr. Sears C. Walker, Assistant in the United States Coast Survey, by instructions from the Superintendent, communicated the substance of his recent Report on the Experience of the Coast Survey, in regard to telegraph operations, as follows: —

The duty of communicating to the Association the experience of the United States Coast Survey, in regard to telegraph operations, has been assigned me by the Superintendent.

The first mention of the electro-magnetic telegraph, in connection with longitude operations, as far as I know, was made, in 1837, by M. Arago to Dr. Morse.

The first practical application of the method was by Captain Wilkes, in 1844, between Washington and Baltimore. Two chronometers, previously rated by astronomical observations in the vicinity, were brought to the two telegraph offices, and were compared together through the medium of the ear, without coincidence of beats. This process is accurate enough for geographical or nautical purposes; but its precision stops short of the mark where the requirements of geodesy begin. In fact, two clocks beating the same kind of time, when placed side by side, cannot be compared together, by the human ear, with sufficient precision for geodetical purposes. The subsequent experience of the Coast Survey has shown, that where several astronomers make independent comparisons of clocks, in this manner, two seconds of arc, or twelve-hundredths of a second of time, is an average discrepancy between their results.

The subject of telegraph operations, for longitude, had engaged the attention of the Superintendent of the Coast Survey, previous to the experiment of Captain Wilkes; but the orders received by me for this purpose, bear date of November 24, 1845. In 1846, the very first season in which two astronomical stations of the Survey were brought in connection by the Morse telegraph lines, the work of connecting them together, in longitude, was commenced in earnest by the Superintendent of the Coast Survey. The coöperation of the National Observatory, as one of the stations, was freely tendered by its Superintendent, Lieut. Matthew F. Maury, U. S. N., and accepted by Prof. Bache. The detail of the operations was left to me. The Comptrollers of the Public Schools of Philadelphia had tendered the free use of their High School Observatory, under the direction of Prof. E. Otis Kendall. An additional station, at Jersey City, was fitted up by the Coast Survey, with astronomical instruments, under the direction of Prof. E. Loomis. The three stations were connected by special junction lines, (built at the expense of the Survey,) with the main Morse line, from Washington to Jersey City, under the Presidency of the Hon. Amos Kendall. Three full sets of telegraph keys and receiving magnets were made for the purpose, at the machine shop of the Coast Survey Office, under Mr. Saxton's directions, and three Morse registers were purchased for the occasion, from Mr. Clark, an instrument-maker in Philadelphia.

Owing to the imperfect insulation of the lines, the connection of Jersey City with Washington failed that year; but the Washington and Philadelphia stations were connected together astronomically on the 10th and 22d of October.

The method of comparison, by coincidence of beats of solar and siderial timekeepers, was not introduced this year; but the equivalent one was employed, namely, the exchange of star signals. These are the dates of instants of the passage of a star over the wires of the eye-piece of the transit instrument, signalized by tapping on the telegraph key at one station, and recording it on the Morse register at both.

Since this operation of making the time of a star's transit legible, on any number of registers, at any number of stations, is likely to come into general use, in geodesy, geography, and hydrography, so long as there remains on the earth's surface any two important points, whose relative longitude is unknown, the origin of the method must be a subject of historical interest.

The idea of substituting for clock signals, which have no immediate relation to longitudes, those of the times of a star's transits, which have such a relation, occurred in the winter of 1845 and 1846, in a consultation between the Superintendent and myself. Which made the first suggestion, I cannot now say, nor is it a matter of much importance to either of us personally. As respects the service in which we are engaged, the origin is a matter of importance. It is a subject of just pride to those engaged in it, that the first conception of the method, and the first practical operation with it, are peculiarly its own. The blank form, No. 2, of telegraph operations, drawn up by me in the spring of 1846, and lithographed for use at the three stations, is headed "Coast Survey," and is described in my lithographed "Circular to the Telegraph Operators." It is dated "Coast Survey Office, September 25th, 1846." In pursuance of this programme, on the 10th of October, the transit of the star 2838 Bailey, over the seven wires of the west transit instrument of the Washington Observatory, was signalized by Lieut. Almy, U. S. N., the officer having charge of that instrument. This transit was noted on the Washington clock, by Lieut. Almy, and also by myself, comparing together, by the ear, the seven key beats, with the clock beats. The same key beats were also noted by Prof. Kendall, at Philadelphia. This was the first practical application of the method of star signals, which is sooner or later to perfect the geography of the globe. The signals

were made visible on the running fillet of paper. The fillet was not then furnished with a visible graduation.

The *personal graduated clock register*, and the *personal register of the star signal*, were both made in an evanescent form on the *auditory nerve* of the ear. Two years later, (1848,) the *visible* was substituted for the *auditory* register, in the month of July. In the month of November, the *automatic visible register* took the place of the *personal visible register*, and constituted the last step in the improvement of the art of telegraph operations for longitude.

Whether we use the *personal auditory*, *personal visible*, or the *automatic visible* register, the theory of the resulting longitudes is the same. They differ only in the superior facility, precision, and permanency of the latter.

This analytical theory, from beginning to end, belongs to the Coast Survey Service. My letter to the Superintendent, on file in the Coast Survey Office, dated October 3d, 1846, contains the *analytical theory* of longitudes by star signals, whether recorded on a *personal auditory register*, as in 1846, a *personal visible register*, as in 1848, or an *automatic visible register*, as in 1849.

I deem it, therefore, but a duty to the service in which I am engaged, to declare, in the presence of the members of the Association, that, with the single exception of the experiment between Baltimore and Washington, by Captain Wilkes, in 1844, I know of no telegraph operations for longitude, and of no step in the improvement, or perfectionment of the art in Europe or America, which have not been the work of the officers proper of the Coast Survey, or of commissioned officers and civilians acting temporarily as assistants.

After this digression, I resume the recital of our experience in the Coast Survey.

In 1846, we connected together, in longitude, the Washington and Philadelphia stations. In 1847, the programme left unfinished in 1846, by the imperfection of the lines, was resumed and completed, and Washington, Philadelphia, and Jersey City, were connected together. On the 27th of July, 1847, the method of coincidence of beats, used so successfully by R. T. Paine, Esq., in the chronometric operations for longitude, in Massachusetts, and by Struve and Airy in their chronometric enterprises, was applied to the telegraphic comparisons of the Philadelphia and Jersey City clocks. This method of coincidences was used in combination with exchanges of star signals in the telegraphic operations of the Coast Survey, in 1848, when the

Cambridge Observatory, under Prof. Bond, and the Stuyvesant station in the garden of Dr. Rutherford, New York, under the direction of Prof. Loomis, were connected together by the Coast Survey. The lines of junction with the main line, under the presidency of the Hon. Francis O. J. Smith, the telegraph apparatus, and the astronomical instruments of the Stuyvesant station, were furnished at the expense of the Survey. During these operations, in July and August, Prof. Bond proposed to substitute the *automatic* instead of the *personal seconds circuit breaker*, and submitted to me a sketch of his plan for effecting the *make* and *break* circuit by the escapement beat. On my special recommendation, by report to the Superintendent, dated August 11th, 1848, an order was given to Mr. Bond for the completion of a *circuit breaker clock* on his plan.

The *automatic clock circuit breaker*, named by Dr. Page, of Washington city; an *electrotome clock*, is not an American invention. It was used more than ten years ago, by Messrs. Wheatstone, Bain, and Steinheil, in Europe. It is due also to Prof. Bache, Mr. Saxton, and Dr. Morse, to say that each of them suggested the use of *clock electrotomes*, in 1846, with particular descriptions of the methods. I was deterred from using them in connection with the observing astronomical clocks at the time, from apprehension of disturbance of their rates.

This apprehension was in reality groundless, and I would here make the remark that all the methods then and since proposed or tried, for *clock electrotomes*, and all the various methods of registering, are precise enough for the purposes of geodesy. Hence, in giving preference to one over another, we should only be guided by considerations of facility and economy of outfit, and of facility of making, reading, and preserving the printed registers.

After concluding the work between Cambridge and New York, in 1848, the month of October was employed by the Coast Survey in connecting Philadelphia and Cincinnati, by means of the O'Rielly Morse lines. The Philadelphia junction line was made by the Coast Survey. The Cincinnati junction line of half a mile in length was erected by Henry O'Rielly, Esq., and presented to the Cincinnati Observatory, (as his subscription towards that establishment. The transit and telegraph instruments for Cincinnati, were supplied by the Coast Survey for Prof. Mitchel.

The labors of the year 1848 comprise some 1800 observed transits of stars, 800 comparisons of chronometers, by coincidences of

beats taken at the stations, 5,000 transits over wires, for determining the personal equations of the officers of the Survey, many thousand exchanges of personal clock signals, and 600 of star transit signals.

If even this prodigious accumulation of statistics was considered a gain of many fold over the old method of obtaining astronomical longitudes, what shall we say of the automatic process employed in 1849, where one night's exchange of star-signals between Philadelphia and the Seaton station, printed automatically on the single sheet of paper now before you, is worth the whole list of statistics collected by the Coast Survey between Philadelphia and Washington, in 1847.

The subject of the invention of the automatic clock register is explained in full in my Report to the Superintendent of the Coast Survey, of December 15th, 1848, which, with his letter communicating it to the Treasury Department, and that of the Hon. Robert J. Walker, communicating both to Congress, have since been extensively circulated by the press.

I will not here allude to the respective claims of Americans for priority or superior excellence of inventions and suggestions, believing that it will be becoming for all of us, to look to the great work that has been accomplished, by our united efforts, rather than to the single share of each.

Feeling the responsibility under which I was acting, I spoke with caution on the subject of the comparative excellence of the automatic printing method; though some of my friends thought that its merits were overrated. I appealed to the experiments that were to be made in the campaigns of 1849 for a test of the new method. That which was then anticipation only, is now reality; and I am able to say, from recent trials, between Cambridge and Washington in January last, and between the Seaton station under my care at Washington, and the stations at Philadelphia under Prof. Kendall, and Hudson, Ohio, under Prof. Loomis, in July and the current month, that the excellence of the new method surpasses all that I ventured to hope for in December last. I then ventured to claim for the automatic printing method a ten-fold gain over the old one. I now find that one transit over one wire is worth four wires by the old method, and that ten transits over wires may now be printed, where one was done before; making a gain by the new or automatic method of some forty-fold. I mean by this the gain from multiplication of transits over wires, and superior precision of each. We cannot in one night obtain the advantage of the average of the meteorological peculiarities of forty.

The various clock *electrotomes* may be thus classified: —

Author.	Date of invention or suggestion.	Description of method.
Wheatstone . . .	1840.	Circular disc on arbor of second's wheel.
Bain	"	Sliding rod moved by pendulum.
Steinheil	Not known.	
Saxton	At launch of Frigate Raritan.	Platinum tilt hammer moved by pendulum.
Saxton	In 1846.	Globule of quicksilver struck by pendulum.
Johnson & Speed .	In 1847.	Platinum tilt hammer struck by teeth of the minute wheel.
Bond	In 1848.	Escapement electrotome.
Mitchel	"	Quicksilver struck by pendulum.
Locke	"	Platinum tilt hammer struck by teeth of seconds-wheel.

I have already remarked that I believe that all these electrotomes will succeed in practice, without injury to the rate of the clock. In making our selection we must look to facility and cheapness of construction, and to uniformity of graduation of the time scale. In the present state of my experience on this subject, I prefer the form now in use at the Seaton station, recently applied by Mr. Saxton to the Hardy dead-beat clock of the United States Coast Survey, imported by its late Superintendent, Mr. Hassler, and by him described in the Transactions of the American Philosophical Society. The electrotome is on the model used by Mr. Saxton several years ago, at the time of the launch of the frigate Raritan, at Philadelphia. The perfect gravity escapement, (resembling, I believe, that which was recently invented by Dr. Locke,) has been substituted by Mr. Saxton for Hardy's springs. Two pounds have been added to the weight of the clock to prevent danger of stopping by increased friction. In this condition Mr. Saxton expresses the opinion that the clock of the Seaton station is the best clock extant. It has broken the circuit for forty days without interruption, and has in no instance deviated in an appreciable amount from its losing rate of one-tenth of a second per day. For a more full understanding of it, I refer you to the accompanying description and drawings by Mr. Saxton.

I have spoken of the *automatic circuit breaking clocks;* it remains to notice the *automatic registers.* The first in order is the registering fillet of Dr. Morse, described in my report of December last. This has no maintaining power. It may be made to run with one winding

for fifteen minutes; but gives uncertain records during the time of winding.

In reading off the dates of the signal electrotomes on the clock electrotome scale, a small strip of paper or of horn, is used, which in ten inches diverges from a point to the extent of a second on the register. This is placed on the fillet of paper, so that its width equals the distance of the clock and signal electrotomes. The tenths of seconds are read off at sight, and hundredths are set down by estimation. A correction for rate of the actual time scale on that of the normal or measuring scale might be applied if we wished; but on experience of a month at the station, we find that the neglect of it brings an error not greater than that of the conjecture, relative to the hundredth of the second, so that in practice we may dispense with the correction altogether.

The next method is the *chemical* method of registering with the main circuit. I have made full experiments with it, and think that the advantage of avoiding the errors of the relay and primary armature time, are more than counterbalanced by the difficulty and irregularity of the action of the current, the indistinctness of the edges of the scale pauses after the paper is dry, and the irregularity of hygrometric expansion of the paper.

In the experiment of measuring the velocity of the hydrogalvanic current, the method will be useful, as it removes all questions relative to *armature* time, though perhaps it substitutes a longer delay, which I may call *chemical action* time.

On the subject of the velocity of the hydro-galvanic current, I would remark that the experiment of January 23d, between Cambridge, Philadelphia, and Washington, indicates a velocity about one-tenth of that of light. This value, though probable, is by no means certain, and I would wish to speak with caution till more experiments are made.

The third form is the register on a disc with concentric circles, as described by Prof. O. M. Mitchel, in his letters to the Superintendent.

Prof. Mitchel's experience of the despatch and precision of the work, confirms my own. I have no doubt that his method of registering is excellent; perhaps not inferior in compactness, precision, and facility of reading off, to the Saxton sheets. I should wish for the opportunity of personal inspection before giving an opinion conclusively on the subject.

The fourth form of the register is Mr. Saxton's invention of this

year. I submit his drawings of the machine. It is somewhat on the plan of his celebrated ruling machine. The cylinder now before the Association contains the culmination of the planet Neptune and the stars near his parallel, printed by me at the Seaton station, Aug. 11, 1849. It might seem that the subject of the place of the planet Neptune is foreign to the purpose of telegraph operations. Such is not the case; for we have used this planet as a fundamental star. I take occasion, therefore, to remark that the observations of the culmination of Neptune on four nights in the month of August at the Seaton station, by Pourtalès and myself, show that my Ephemeris, published by Prof. Henry in the Smithsonian Contributions to Science, agrees with the heavens within half a second of arc. From this close agreement it may be inferred that if the Neptune of Prof. Peirce's theory and my elements were conceived to be a planet, placed side by side in the heavens with the true one ever since its discovery, the two would form a double star of an order so close that not even the great Cambridge refractor could detect their duplicity.

An objection has been urged to the Morse registering fillet, that it is too voluminous for the quantity of matter recorded. This objection and that of expensiveness, occur with more force to the metallic cylinder, however accurate be its indications. To remedy this evil, Mr. Saxton has contrived a sheet of paper which encloses the cylinder and lasts for about two hours of constant work. The sheets and registering fillets now submitted for the inspection of the Association, contain the comparison of the printed record of the culmination of the stars in the Dolphin. The Saxon sheet, the chemical fillet, and the Morse fillet, are triplicate records of the same identical star signals. The result of the reading, as far as experiments have been made, is, that all kinds of registers at the same place read alike. It is worthy of remark, that these registers contain the printed record of the transits of both components of the double star Gamma Delphini, printed with ease on each of the forty-five wires of the Wurdeman's diaphragm, making ninety imprints in a culmination.

From my experience in printing the transit of this pair of double stars, I am led to the conclusion, that four stars forming a quadruple star, when at proper distance, may all be printed at the time of their transit over a diaphragm of fifty wires, making two hundred imprints for one transit, a rapidity of playing on the key far below that of good execution on the piano.

The other sheet before you contains the longitude between the Sea-

ton station and Philadelphia. It was registered at Washington. The Morse fillet on the reel now before you is the duplicate Philadelphia register.

This work was performed last Monday night. You will notice a curious occurrence on several occasions that night. Three distinct telegraph operations were going on at once. The Seaton station clock was graduating the time scale for both stations. Two stars differing in right ascension by about the longitude of the places, were in the two telescopes at the same time, and the imprints of the transits are interspersed, without confusion on the register. Since one wire at each station gives the longitude of the places, we can find cases where the work of a single second has effected this purpose better than a year's work could do without the telegraph.

The diaphragms of nine and eleven tallies (45 and 55 wires,) made by Mr. Wurdeman, formerly the Mechanician of the Coast Survey, are so nearly perfect in their structure that the probable error of his locating of any single wire, in reference to a normal location with equal intervals, is only four hundredths of a second of time. This precision is a matter of great importance in the use of a diaphragm of so many lines, whose equatorial interval, unless in the case of known symmetry, would need a special determination.

Of all the different kinds of registers here alluded to, I prefer the sheet of Mr. Saxton. One sheet filled on both sides, or two pages, will contain an ordinary night's work. A year's work will make a book of some three hundred pages, on the margin of which may be entered the ordinary remarks for an observing book, relative to the state of the level and meteorological instruments, name of stars observed, and instrumental deviations.

If folded up, or bound and put away for a century, the reduction of the work will then be as easy as at first.

In fact, we may, with the metallic cylinder, electrotype the plate; or, using copper, we may print from it without. And, in the case of the paper sheet, instead of Saxton's graver, with Indian ink, we may employ a pen, with lithographic ink, and multiply copies at pleasure, whenever we choose. When we consider the compactness of the register on Saxton's sheet, we may perhaps find that the publication of transit observations will best be made by the lithographic process, applied to the printed telegraph sheets; thus giving to the world the printed record with all the accuracy of a daguerreotype. The registering fillet now exhibited to the Association, contains the culmination

of both limbs of the moon, printed by myself, on the 3d of August last, on 35 wires of the diaphragm. By the mean of the results, the probable error of the imprint of a transit of single limb, over a single wire, is the sixteenth of a second; whereas, in 1846, with the great Washington Equatorial, and a power of 300, I found that, with the old method, my probable error, by 66 trials, was twice as great, namely, the eighth of a second. Thus it appears that the measure of precision is twice, and the weight four times, as great, in the new method, as in the old.

No labor of training for the work is needed. Master Langton, the youngest Assistant at the Seaton station, printed the transits of four stars, on the 18th of July, for his first trial. The fourth transit is on the register now before you. On reading off Master Langton's imprints, we find them as accurate as any of our work, and far more so than that of the most experienced observer by the old method.

A hundred wires is a high estimate for a night's work of an observatory, by the old method. I have printed fifteen hundred wires, without fatigue, in one night, by the new. Since each wire is worth four of those of the old method, we have six thousand to one hundred, or sixty to one, as the relative efficiencies of the night's observations.

When we reflect, that the probable error of one transit, over one wire, is only the sixteenth of a second, and that with five wires it is only a thirty-sixth part, or three hundredths of a second, it is manifest that one tally, or five wires, is ample for all ordinary work. In fact, one wire is sufficient for most of the purposes of astronomy. I have been led, on consideration of all the facts known from the experience of the Coast Survey, at the Seaton station, to make the following remark relative to the precision of our work, after proper adjustment of the transit instrument, or measurement of its deviations from a normal state: — *The printed transit of a fundamental star over any one wire of Wurdeman's diaphragm, and that of a star, planet, or comet, whose place is sought, over another wire — both reduced to the centre, on the supposition of uniformity of interval — give the place of the object sought, with a precision not much below that on which rest the present elements of all the bodies in the solar system.*

Adjourned.

B. A. GOULD, Jr., *Secretary.*

Fourth Day, August 17, 1848.

SECTION OF NATURAL HISTORY, GEOLOGY, &c.

Prof. HALL was appointed to the Chair.

NEW SPECIES OF FOSSIL MYLIOBATES, FROM THE EOCENE OF SOUTH CAROLINA, AND NEW FOSSILS FROM THE CRETACEOUS, EOCENE, AND PLIOCENE OF SOUTH CAROLINA, ALABAMA, AND MISSISSIPPI. BY DR. R. W. GIBBES.

Dr. GIBBES described two species of Myliobates, which differ from previously described species — one he dedicates to its discoverer, Mr. F. S. Holmes, of Charleston, and calls it Myliobates Holmesii; the other he calls, from the position of its plates, Myliobates transversalis.

He then drew the attention of the Association to several new fossils from the *cretaceous* and *tertiary*, which had not been previously noticed; among which are:

CRETACEOUS of Alabama.

Teeth of *Ptychodus polygyrus*, *Agassiz.*

EOCENE of South Carolina and Mississippi.

A tooth of a large Cetacean, probably *Physeter*.
Cetotolites of a huge Cetacean.
Otolite of *Basilosaurus cetoides*, *Owen.*
Teeth of *Crocodilus* — (undescribed.)
Tooth of *Equus*, resembling E. plicidens, *Owen.*
Tooth of *Equus curvidens*, *Owen.*
Jaw of *Cœlorynchus.*
Rib and vertebræ of *Manatus.*
Bones of Birds.

A specimen of a true Carcharias, and one of Carcharodon leptodon, (*Agassiz*,) not previously noticed in the United States.

Teeth of *Basilosaurus cetoides*, from Jackson, Miss.
Cast of a *Cancer*, from Jackson, Miss.

PLIOCENE of South Carolina.

A tooth of *Physeter*.

Vertebræ and ribs of large *Cetacea*.

Tooth of *Equus americanus*, *Leidy*.

Tooth of *Sphenosaurus clavirostris*, *Agassiz*.

Aphophyseal bones of *Platax*, *Agassiz*.

Dr. GIBBES, in presenting the individual specimens, gave a short notice of their position geologically, and observed that, as a special description of them would be given in the journals, he would not detain the members with more particular details.

Prof. AGASSIZ expressed his great interest in the fossils exhibited, and especially of the Cetacea, not before observed; and alluded particularly to the discovery of *Birds' bones*, in the Tertiary, as worthy of special attention.

In relation to the bones of *Platax*, he observed that it was quite interesting to find in a fossil state, on the slope of the Atlantic, the bones of genera of fishes extensively distributed in the present Atlantic Ocean. It was an evidence more that they did not all die out in the older deposits, but continued through a succession of epochs.

ON FIORD'S EVIDENCE OF A CHANGE OF LEVEL. BY J. D. DANA.

[Not received.]

ON THE HABITS OF AMPHIUMA IN A STATE OF CAPTIVITY. BY DR. J. L. LE CONTE.

Dr. LE CONTE gave a short account of the habits of a specimen of Amphiuma, which had been kept alive for eight years, by Mr. Zimmerman. It did not appear to make use of its branchial organs in respiration, as it frequently emitted bubbles of air from its nostrils, and rose to the surface of the water for a fresh supply. It was kept in a shallow basin of water, and often, when the water was rendered impure by the exudations from its body, the animal came out upon the floor, where it would remain for several hours, or until the water was changed. It was fed with earth worms and small shreds of raw meat. It was very voracious, snapping its jaws together with great violence; but the food was never seized except under the water. Dr. Le Conte suggested that the branchial openings would be of great

service in letting out the water, which was taken in the mouth with the food.

In this animal the branchial apparatus was reduced to a most rudimentary condition, and he thought that its object was not functional, as it was entirely too insignificant in development to be of any service in respiration. We must therefore look beyond mere material teleology for its explanation. It was intended to illustrate the connection between the fishes and reptiles, as parts of the same great series, and to show simply that both were constructed on the same typical plan. Continually, in the animal kingdom, structures are retained which are of functional use only in inferior animals, or in the younger states of the same individual.

On some Curious Habits of a Species of Asilus. By Dr. J. L. Le Conte.

Dr. Le Conte then noticed the habits of Asilus vertebratus, *Say*, in seizing its prey. This Dipterous insect inhabits the plains adjacent to the Rocky Mountains, where it frequently attacks insects much larger than itself. He noticed particularly the effects produced by it on the large Cicindelæ of that region, (C. pulchra, obsoleta, *Say*,) which were seized while flying in the air, and were always killed before the Asilus could be driven off. Vital action appeared to cease instantly, and this was evidently caused by a very virulent poison, as the insect attacked was always found in a completely relaxed state, nor did the ligaments become stiff again until the specimen was dried. This was not owing to the sudden introduction of a sharp pointed instrument (such as the beak of the Asilus) into the vital parts of the insect, for Dr. Le Conte had taken a large pin and introduced it between the head and thorax of Cicindelæ, and even moved it about, without producing any apparent effect, and when the insect finally did die, the limbs were not relaxed. He concluded by pointing out the apparent connection of these phenomena with the great irritation produced on our own persons, by very small insects, also belonging to the order Diptera, Simulium, (gnats, sand-flies, &c.).

Fourth Day, August 17, 1849.

AFTERNOON SESSION.

THE Association met in General Session, at half-past four, in Harvard Hall.

The first paper was read by Prof. HORSFORD, as follows: —

NOTICE OF SOME EXPERIMENTS ON THE ELECTRICITY OF A PLATE OF ZINC BURIED IN THE EARTH. BY ELIAS LOOMIS, Professor of Natural Philosophy in the College of New Jersey. Read by Prof. HORSFORD.

IN October, 1842, Mr. Alexander Bain discovered that a plate of zinc buried in the earth, and connected by a wire with a copper-plate similarly buried, at the distance of a mile, produced an electric current with which various electro-magnetic experiments were successfully performed. I have lately performed a series of similar experiments for the purpose of ascertaining the circumstances which determine the intensity of the current, and of discovering to what extent this intensity might be increased.

On the 15th of May last, I took a plate of sheet zinc, twelve inches by sixteen, and having soldered to it a copper wire sixty feet in length, buried it two feet beneath the surface of the earth, on the north side of the Philosophical Hall of the college of New Jersey. A plate of sheet copper, nine inches square, was buried in a similar manner, twenty-seven feet from the former. The wires from these plates being together one hundred and fourteen feet in length, extended to the upper story of the hall, and were connected with a small galvanometer made by E. M. Clark, of London. The needle (which was two and three-eighths inches in length,) was deflected with great violence, and finally settled at sixty-six degrees. The current of positive electricity flowed through the earth from the zinc to the copper-plate, and thence through the air back to the zinc plate.

When one of Morse's receiving magnets was substituted for the galvanometer, the armature was readily attracted, and after adding a single cup of Grove's battery for the local circuit, the register worked promptly and efficiently.

I moistened a strip of paper with a solution of Iodide of Potassium and starch. Upon introducing the paper into the circuit, the Iodine immediately made its appearance upon the positive pole. Morse's

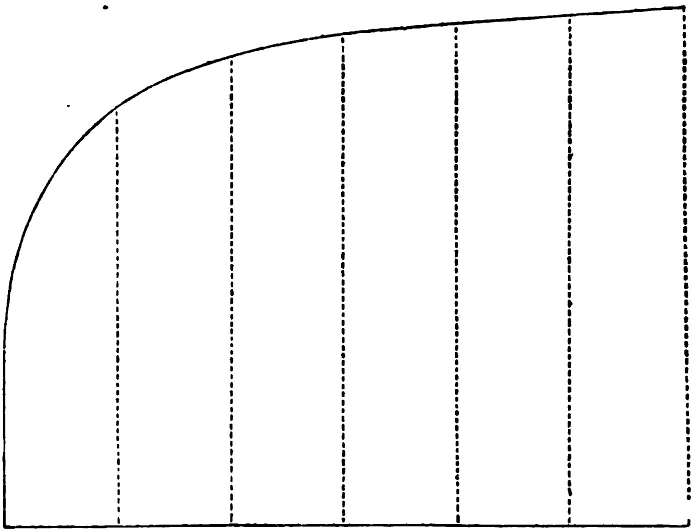
telegraphic characters were promptly stained upon the paper, so as to admit of telegraphing with tolerable rapidity.

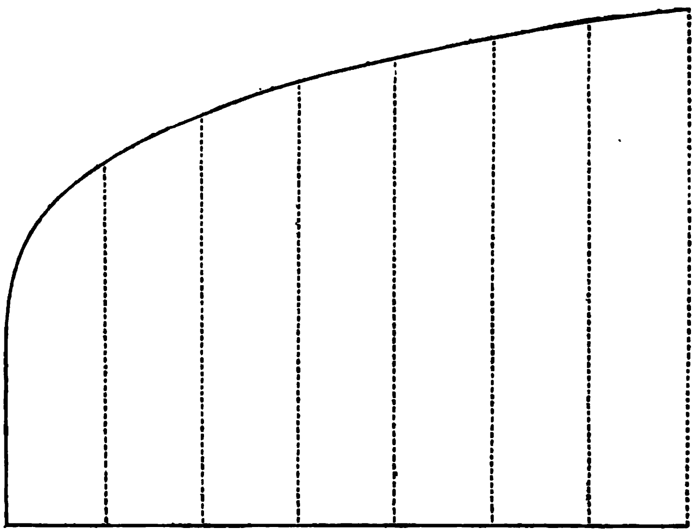
I moistened a strip of paper with a solution of Prussiate of Potash in Nitro-muriatic acid. This was readily stained a deep blue, when the current was transmitted through a steel point. Morse's telegraphic characters were marked upon the paper with promptness.

On the first of June, the galvanometer indicated precisely the same intensity of current as on the day the zinc plate was buried. I now buried a second zinc plate, twenty inches square, by the side of the former one, at the depth of two feet beneath the surface of the earth, and connected the two plates by a short wire. The galvanometer now settled at seventy-two degrees. Thus it appears that by the addition of the second plate, which was considerably larger than the first, the strength of the electric current had been increased about one-third.

I took a copper wire 760 feet long, and attached to one extremity a plate of sheet copper 48 inches by 14, which I immersed in a well 475 feet distant from the zinc plate. About 250 feet of the wire was stretched around the Philosophical Hall; the remainder was suspended in the air over the tops of the houses, and was insulated by glass. Upon connecting this wire with that from the zinc plates, the galvanometer settled at 74¾ degrees.

In order to determine the influence of the size of the copperplate upon the intensity of the current, I divided the sheet in the middle, leaving a plate 24 inches by 14 immersed in the well. The galvanometer stood at 71½ degrees. I thus proceeded to divide the copperplate, until I had reduced it to three inches by a half inch, the galvanometer being recorded at each observation, after which the plate was entirely removed. I then withdrew the wire gradually from the well, until only three inches of it remained immersed in the water, and noticed at each step the effect upon the needle. Assuming now that the intensity of the current is measured by the tangent of the angle of deviation of the galvanometer, we find that a copperplate one inch wide and two inches long, yields one-half the current of a plate having *nine times* the amount of surface; and it yields one-fourth the current of a plate having *three hundred and twenty times* the surface.





I now took a strip of sheet zinc, one-tenth of an inch wide and twenty inches long, and having soldered to it a copper wire sixty feet in length, inserted it vertically in the ground near the Philosophical Hall. One end of the wire before mentioned, 760 feet in length, was dropped into the well 475 feet distant, without any plate attached to it. Upon connecting the two wires, the galvanometer settled at $38\frac{3}{4}$

degrees. This current was sufficient to work the telegraph register with promptness and efficiency.

The following experiments were made to determine the influence of the length of the conducting wire upon the intensity of the current. I attached a copper-plate 14 inches by 24, to the end of a long wire, and immersed it in the well before referred to. When the zinc plate, twenty inches square, was connected with it, the length of wire in the circuit being 570 feet, the galvanometer settled at seventy degrees. When the length of wire was increased to 940 feet, the galvanometer settled at 69½ degrees. When the length of wire was increased to 1450 feet, the galvanometer settled at 69¼ degrees. Thus it appears that when the length of the circuit was doubled, the intensity of the current was but slightly impaired, which seems to favor the idea that the current thus generated might be employed for telegraphing to considerable distances. Mr. Vail succeeded in telegraphing from Washington to Baltimore with such a battery. He does not expressly mention the size of the plates ; but it is inferred that the copperplate was five feet long, by two and a half feet wide, and the zinc plate had probably about the same dimensions.

The following experiments were tried to determine how far the intensity of the current could be increased by multiplying the number of galvanic elements. I buried a plate of zinc, six inches square, in the earth, at a distance of twelve feet from the well before mentioned. A plate of copper, six inches square, being attached to a wire and dropped into the well, the galvanometer settled at 26 degrees. I then buried a second copper plate, six inches square, at the distance of one inch from the first zinc plate, and connected it by a wire with a second zinc plate, which was immersed in the well by the side of the copperplate, and separated from it to the distance of half an inch, by interposed cork. The galvanometer settled at 44 degrees. The tangents of 26 and 44 degrees are in the ratio of one to two. The intensity of the current was, therefore, doubled by the addition of a second pair of plates.

I removed the second copperplate to the distance of five inches from the zinc which was buried in the earth, when the galvanometer settled at 33 degrees. I then interposed between the copper and zinc a third pair of plates, of the same dimensions, connected by a strap of copper. The galvanometer settled at 40½ degrees. This experiment did not afford much encouragement for increasing the number of plates beyond two pairs of elements. By bringing the plates a little

closer together, a slight increase of effect would have been obtained; but although the experiment was several times repeated, nearly the same advantage appeared to be lost in every instance by the separation of the second copperplate from the first zinc, as was gained by the interposition of the third pair of plates.

I then immersed a battery of three pairs of plates in the well, the outer copperplate being connected by a wire with the zinc plate in the earth, and the outer zinc plate being connected with the copperplate buried one inch from the zinc. The intensity of the current was less than when a single pair of plates immersed in the well was connected with a pair of plates buried in the earth.

The details of the experiments, of which the preceding is but a brief synopsis, will be published hereafter, provided they should be thought of sufficient interest.

This was followed by remarks from Prof. HENRY, Dr. HARE, and Prof. H. D. ROGERS.

Prof. H. D. ROGERS mentioned, that in a series of experiments published several years ago, in Silliman's Journal, by Prof. W. B. Rogers and himself, results were stated which would be found, he thought, to possess some practical interest, in relation to the conditions which influence the production of electric tension. The experiments referred to, seemed to prove that a far more intense electric action is procured, when the surface of the zinc element exposed to chemical action bears a small proportion to the copper surface, than when their areas are reversed. They likewise indicated a remarkable exaltation of action when the acid or other chemical agent was heated; and it was found that after a progressive declension of the electric energy attendant on immersion, a quick restoration of power could be effected, by withdrawing the plates for a brief interval, and wiping them.

Prof. HENRY read a letter received from the President of a Telegraph Company, mentioning a phenomenon that had occurred during a thunder storm. The letter was made the basis of a communication from Prof. HENRY, in relation to atmospheric electricity, which was followed by a discussion, in which Profs. HENRY, HARE, and JOHNSON, and Dr. C. T. JACKSON, took part.

Remarks on the Supposed Connection between Cholera and Electricity. By Dr. Hare.

Agreeably to a statement, recently republished in some respectable newspapers, a French electrician finding an unusual difficulty in producing electricity by his electrical machine, conceived it to be demonstrated that the difficulty did not arise from any imperfection or impediment in the apparatus employed. Hence it was inferred by him, that the failure of the machine to display its wonted power, might result from a deficiency of electricity in the atmosphere; and, moreover, that the prevalence of cholera might be the consequence of the same deficiency.

The idea that there can be a greater or less quantity of electricity in the atmosphere, seems to be irreconcilable with any accredited doctrine; or with any to which the author can be presumed to adhere from the language employed.

This language evidently assumes electricity to be a material principle, of which, more or less may exist in the atmosphere throughout an extensive region.

Agreeably either to the Franklinian doctrine of one fluid, or Dufay's doctrine of two fluids, the efforts of the electrician to produce electricity are not in the least dependent on the *quantity* of electric matter present; but on the competency of the apparatus employed to destroy the equilibrium, to which, when undisturbed, that matter always tends. When an electrical machine is in successful operation, so as to charge its oppositely excited conductors to a maximum, there is no more electricity present than prior to the process; since, whatever has been gained by one surface, must have been lost by the other.

It is true that agreeably to the theory of Franklin, the sky and the terrestrial surface may be oppositely charged so that one may be negatively, the other positively electrified, in which case the terrestrial surface may be deficient and the sky surcharged, but this would not prevent an electrical machine resting on the earth from causing the fluid to be redundant on one of its insulated conductors, and proportionably diminished upon the other.

According to Dufay there is as much electricity in a negatively excited body as in one positively excited; the difference is, as to kind, not as to quantity. There is as much more resinous electricity in the negative conductor, as there is less of the vitreous; while in

like manner, upon the positive conductor, the vitreous surcharge compensates the resinous deficiency.

Admitting, therefore, the observations of the electrician to have been so far correct, that he actually found the friction of glass less capable of producing electricity during a period when cholera prevailed, than at others, either consistently with neither doctrine can it be the consequence of a deficit of electric matter. It can only have arisen from an unusual insusceptibility of the media in which electric disturbance is producible, to assume those opposite electrical states which it is the province of the electrical machine to create.*

According to the electrical theory which I have advanced, opposite electrical states are not the consequence of any redundancy or deficiency of electric matter; but are owing to opposite states of polarity produced in an etherial medium which pervades the universe, and probably identical with that of which the undulatory affections are inferred to be the cause of light. The working of an electrical machine causes two atmospheres of oppositely polarized ethereal particles to exist severally on the conductors. The negative poles of the one set of particles, forming the positive atmosphere, are directed towards the positively polarized surface of the *positively* excited conductor, the positive poles of the set of ethereal particles forming the negative atmosphere being directed towards the *negatively* polarized surface of the negatively excited conductor. At the same time the particles of atmospheric air, or of any other light matter, which may be associated with the polarized atmospheres, assume a corresponding polarity.

This association, together with its polarizing influence, is seen where four artificial scalps abundantly furnished with hair, are placed upon insulated conductors; so that two of the scalps may be electrified positively and two negatively. Under these circumstances, the hair similarly excited will be reciprocally repulsive; that dissimilarly

* Agreeably to Faraday's researches, and general experience, we have reason to believe that all particles of matter are endowed with one or the other of two species of polarity. This word polarity conveys the idea that two terminations in each particle are respectively endowed with forces which are analogous, but contrary in their nature; so that of any two homogeneous particles, the similar poles repel each other, while the dissimilar attract; likewise when freely suspended they take a certain position relatively to each other, and on due proximity, the opposite polar forces, counteracting each other, appear to be extinct. When deranged from this natural state of reciprocal neutralization, their liberated poles react with the particles of adjacent bodies, or those in the surrounding medium. Under these circumstances, any body which may be constituted of the particles thus reacting, is said to be polarized, or in a state of polarization.

excited will be reciprocally attractive. Individually the hairs upon either scalp, being of necessity similarly electrified, strive to separate from each other, diverging like radii from a centre; *all* the hair on any one scalp, will attract or repel *all*, on any other, accordingly as the electricity received may be similar or dissimilar.

The reaction between the hair, as above described, very much resembles that between the catenated filaments formed by iron filings when sifted on paper over two horseshoe magnets, the poles of which are relatively to each other at suitable positions with the requisite proximity. Hence, as both are attended by polar affections, the term polarity or polarization may in either case be employed to designate the phenomena.

Yet I conceive the one to be due to an intestinal arrangement of the ferruginous particles, the other to a superficial affection of the insulated conductor, and of the hair consequent to the polarization of the etherial atmosphere within which they are involved.*

If, as I have urged, the province of an electrical machine be to produce waves of polarization in an etherial medium, always equally abounding in all space, evidently any incompetency which may be discovered in the machine, cannot be the consequence of a diminution of electricity in the atmosphere, but must be owing to some change in the properties of the polarizable surfaces of the glass or cushion, or of the surrounding ethereal or gaseous media.

It has long been known that substances are capable of different states; of which the most familiar exemplification is afforded by carbon. This element may be seen in the extremely combustible state of a pyrophorus, or the almost incombustible state of plumbago; in the opaque form of anthracite or charcoal, or the transparent form of diamond. Many elements which do not unite with each other in one state, in another combine energetically. This is eminently true in the case of carbon with nitrogen; of nitrogen with hydrogen; nitrogen with oxygen; chlorine with carbon; and hydrogen with carbon.

This state of susceptibility of union has been called the *nascent* state (from *nasco*, to be born) because bodies are usually endowed with it at the moment of apparent birth, as on being just liberated from combination. Dr. Draper has made us acquainted with a permanent change of state in the case of chlorine, in which a greater

* In order that the reader may understand the grounds on which this theoretic exposition is founded, attention may be directed to the series of Essays by Dr. Hare, published in Silliman's Journal, particularly from paragraph 29 to paragraph 57 inclusive.

susceptibility of combining with hydrogen is permanently acquired. Chlorine does not react with hydrogen when a mixture of these gases is exposed to diffuse daylight. Yet a pencil of the solar rays, on entering such a mixture, causes forthwith explosive inflammation. Nevertheless, agreeably to Draper, if, after chlorine has been exposed to sunshine, a mixture of it be made with hydrogen, reaction ensues on exposure to a feeble light. This property of undergoing a change of state has been called *allotropism*, by Berzelius.

Faraday has shown that the electric spark varies in its length, its form, and color, with the *nature* of the gas in which it may be made to take place. During the charging of the conductors, (of which the knob or knuckle, receiving the spark officiates for one,) the intermediate gaseous particles yield to the polarizing influence, and being (as I conceive) together with the associated ether, brought to the highest state of polarity of which they are susceptible, are suddenly depolarized by being allowed to meet and neutralize each other in a discharger, or other competent conductor, such, for instance, as any part of the animal frame. It is during this return to *statu quo*, that the shock is experienced by the person of the operator when forming a part of the circuit.

But if, according to Faraday, the charging and discharging of bodies with opposite electricities, be influenced by the *nature* of the intervening gaseous particles, may they not be influenced also by the allotropism of those particles? So powerful is the influence of the working of an electrical machine upon the media around, that a remarkable odor is always perceptible. Moreover, the air seems to have acquired properties so remarkable and peculiar, that a distinguished philosopher, Schönbein, inferred that they were ascribable to its containing a peculiar elementary body which he called *ozone*.

Schönbein subsequently found that a similar change might be affected in air by the presence of phosphorus and other means. Nevertheless, all efforts to isolate any substance endowed with the requisite properties failed, and facts were noticed inconsistent with the existence of any such body as ozone.*

* The following experiment, for which we are indebted to the distinguished philosopher, De La Rive, seems to be conclusive as to the fact, that ozone may be generated from isolated oxygen.

Into a glass tube a platina wire was secured by passing the glass over it, at the same time closing one of the orifices. One end of the wire thus secured, was made concentric with the axis of the cavity, and to project within it a convenient distance. Chlorate of Potash fused so as thoroughly to expel moisture, was introduced into the cavity of the

If, according to Faraday, the intervening, or circumambient gaseous particles participate in, and modify the phenomena which take place, during charge and discharge, so that the polarity to them imparted, and its effects, are essentially and actively influential upon the result, varying it according to their nature, — if the odor ascribed by Schonbein to ozone be due to a change in the state of the atmospheric oxygen in the vicinity, ozonification may be necessary to electrical excitement. Of course, should there be any allotropism of oxygen, capable of rendering it less susceptible of ozonification, or, in other words, of the polarization which electrical excitement requires, such an allotropic state may, at the same time, produce a greater tendency to disease, so that lesser exciting causes may cause sickness.

This idea may be illustrated by an analogy.

Suppose sulphur, phosphorus, and carbon, in their different states, to be subjected to a rise of temperature while exposed to the oxygen of air. Every chemist must be aware that carbon, in the state of pyrophorus, would take fire at the lowest temperature; phosphorus would burn next; then sulphur, tinder, charcoal, anthracite, plumbago, diamond; each in succession as named.

Were sparks emitted from flint and steel to fall on each of the combustibles above mentioned, it is probable that carbon, in the state of tinder or pyrophorus, would be ignited first.

Electrical discharges would also more readily ignite some of these forms of matter than others. They would not set fire to tinder as readily as the spark from flint and steel, but would inflame hydrogen or any of its gaseous compounds when the other would fail.

As carbon is more susceptible of the spark from the flint and steel, and hydrogen of that produced by electricity, so we may suppose a diversity in the human organization, as respects its susceptibility of being affected by ozonification, by animalcules, fungi, or any other supposed causes of epidemical sickness.

tube. Heat being applied, the tubular cavity became replete with evolved oxygen, by which the air was expelled through a beak, into which the open end of the tube had been drawn, after introducing the chlorate. Another platina wire being introduced through this beak, was secured by fusion of the glass, so as to leave an interval between its termination and that of the other wire, suitable for the transmission of a series of electric sparks. The result was, that the oxygen while thus isolated from all objectionable matter, and subjected to such a series, acquired the odor and properties ascribed to ozone. Hence, Berzelius, with a great many others, as well as myself, inclined to the opinion, that ozonification might be accounted for, by supposing a change of state to be induced in oxygen.

A general rise of temperature has the greatest analogy with allotropism; fungi, or animalcules, with the varieties of fire, producing sparks.

Independently, however, of my being as yet incredulous of the fact, that there has been any difficulty of producing electricity, during the prevalence of cholera, which might not have been traced to imperfection or impediments, it must be evident that the most that our knowledge can justify, respecting any association between electrical susceptibility of polarization, and the causes of cholera, is a *possibility*. There is no evidence which can render such an association probable.*

Adjourned.

E. N. HORSFORD, *Secretary*.

Fifth Day, August 18, 1849.

GENERAL SESSION.

The Association met at 10 o'clock, A. M., pursuant to adjournment. After reading the report of the last meeting by the Secretary, it was moved and carried that the reading of the reports of the Secretaries of the Sections be dispensed with.

The following persons were then nominated by the Standing Committee, and unanimously chosen members of the Association: — Mr. John M. Ordway, of Roxbury; Mr. Charles Girard, of Cambridge; Mr. Increase A. Lapham, of Milwaukie, Wisconsin; and George S. Hillard, Esq., of Boston.

The following resolutions offered by Dr. Gray, in behalf of the Standing Committee, were adopted:

Resolved, That the Local Committee of Cambridge be requested to aid the Secretary in the revision of the minutes and reports for publication in the proceedings.

Resolved, That members be requested to furnish abstracts of their papers, and revisions of the daily reports of their remarks, to the Secretary, within one fortnight from the adjournment of this meeting.

* Since the above was communicated, it has occurred that the odor and phosphorescence of quartz stones, when rubbed together, may be a case of ozonification.

The following resolutions, submitted by Dr. LE CONTE, were by the Standing Committee recommended for adoption:—

Resolved, That the Committee appointed last year for the purpose of memorializing Congress on the importance of scientific exploration, is hereby continued for the present year.

Resolved, That in addition to the duties prescribed at our former meeting, the Committee be requested to present a memorial to Congress on the importance of making general the results of the Exploring Expedition, by distributing to different learned Societies of our country series of the duplicates at present lying useless in the cellars of the Patent Office. Adopted.

The Standing Committee further reported that it was expedient to subdivide each of the two Sections into two other Sections, in order that the business of the Association might be expedited.

On motion, it was voted that the invitation of the Faculty and Corporation of Yale College, to meet at New Haven for the next session, be accepted.

Prof. ROGERS then suggested that an intermediate Spring session might be holden at some place in the southern section of the country, with advantage to the interests of the Association.

Dr. GIBBES followed in support of the same, and in behalf of the naturalists and citizens of Charleston, S. C., invited the Association to meet in that place in March next. The whole subject was then referred to the Standing Committee.

After the reading of the programme, the Association adjourned to meet in Sections.

E. N. HORSFORD, *Secretary*.

Fifth Day, August 18, 1849.

SECTION OF MATHEMATICS, PHYSICS, AND ASTRONOMY.

THE meeting was organized by the election of President EVERETT as Chairman for the day, and B. A. GOULD, Jr., as Secretary.

Mr. SEARS C. WALKER addressed the Association on the subject of a new analogy in the periods of rotation of the primary planets, discovered by Daniel Kirkwood, Esq., of Pottsville, Pennsylvania.

The subject of my present communication is contained in a letter

of Mr. Daniel Kirkwood, of Pottsville, Pennsylvania, dated July 4th, of this year.

The Secretary then read Mr. Kirkwood's letter, as follows: —

Pottsville, Pa., July 4, 1849.

SEARS C. WALKER, Esq.,

Dear Sir, — Knowing the great interest you feel in astronomical inquiries, I take the liberty of submitting the following paper to your consideration, and respectfully soliciting your opinion as to the problem which I have been attempting to solve. Is it, or is it not, deserving of further investigation? Whatever may be your decision, as I have the fullest confidence in your judgment, I shall at once acquiesce. Wishing to be as brief as possible, I will not trouble you at present with any statement of the considerations which suggested my hypothesis.

While we have, in the law of Kepler, a bond of mutual relationship between the planets, as regards their revolutions round the sun, it is remarkable that no law regulating their rotations on their axis has ever been discovered. For several years, I have had little doubt of the existence of such a law in nature, and have been engaged, as circumstances would permit, in attempting its development. I have at length arrived at results, which, if they do not justify me in announcing the solution of this important and interesting problem, must at least be regarded as astonishing coincidences.

Let P be the point of equal attraction between any planet and the one next interior, the two being in conjunction; P′, that between the same and the one next exterior.

Let also D = the sum of the distances of the points P, P′, from the orbit of the planet; which I shall call the diameter of the sphere of the planet's attraction;

D′ = the diameter of any other planet's sphere of attraction found in like manner;

n = the number of sidereal rotations performed by the former during one sidereal revolution round the sun;

n' = the number performed by the latter; then it will be found that

$$n^2 : n'^2 :: D^3 : D'^3; \text{ or } n = n' \left(\frac{D}{D'}\right)^{\frac{3}{2}}$$

For the sake of convenient reference, I subjoin the following tables. The masses of Venus, the Earth, Mars, Jupiter, and Saturn, are taken from your edition (1845) of Sir John Herschell's Treatise on Astronomy. Those of Mercury and Uranus correspond with my hypothesis, and are nearly identical with the most recent and reliable deter-

minations of astronomers. In other words, the mass of Mercury is very nearly a medium between the two estimates of Encke,* while that of Uranus is more than $\frac{17}{18}$ths of Struve's mass, $\frac{1}{26860}$ found by observations on the satellites.† The mean distances not being given in miles in Herschell's Treatise, I have used the table of distances in the Astronomy of Professor Norton. For Mars's period of rotation ($24^h\ 37^m\ 20^s.6$.) I have adopted the recent determination of Prof. O. M. Mitchell. (*Sid. Mess.*, *Vol. I.*, *p.* 52.)

TABLE I.

Planet's name.	Mean Distance from the Sun in miles.	Mass.	Square root of Mass.	No. rotations in one Sid. Period.	Log.
Mercury,	36,814,000	277,000	526.3	87.63	1.942653
Venus,	68,787,000	2,463,836	1569.6	230.9	2.363424
Earth,	95,103,000	2,817,409	1678.5	366.25	2.563777
Mars,	144,908,000	392,735	626.7	669.6	2.825815
Jupiter,	494,797,000	953,570,222	30879.8	10471	4.019988
Saturn,	907,162,000	284,738,000	16874.1	24620	4.391288
Uranus,	1,824,290,000	35,186,000	5931.5		

The points of equal attraction between the planets severally (when in conjunction) are situated as follows: —

II.

	Miles from the former.	Miles from the latter.
Between Mercury and Venus,	8,029,600	23,943,400
" Venus and Earth,	12,716,600	13,599,400
" Earth and Mars,	36,264,600	13,540,400
" Jupiter and Saturn,	266,655,000	145,710,000
" Saturn and Uranus,	678,590,000	238,538,000

It will be seen from above, that the diameter of the earth's sphere of attraction is 49,864,000 miles. Hence the diameters of the respective spheres of attraction of the other planets, according to my empirical law, will be found to be as follows: —

	Diam. of Sphere of Attr.	Log.
Mercury, ...	19,238,000	1.283704
Venus, ...	36,660,000	1.564218
Mars, ...	74,560,000	1.872479
Jupiter, ...	466,200,000	2.668594
Saturn, ...	824,300,000	2.916127

* See Prof. Encke's letter to Mr. Airy, dated Dec. 20, 1841.

† Edinburg Phil. Journal, for July, 1848.

Remarks. The volumes of the sphere of attraction of Venus, Mars, and Saturn, in this table, correspond with those obtained from Table II.; that of Mars extending sixty-one million miles beyond his orbit, or to the distance of two hundred and six million miles from the sun. This is about two or three million miles less than the mean distance of Flora, the nearest discovered asteroid. That of Mercury extends about eleven million miles within the orbit; consequently, if there be an undiscovered planet interior to Mercury, its distance from the sun, according to my hypothesis, must be less than twenty-six million miles. Jupiter's sphere of attraction extends only about two hundred million miles within his orbit, leaving eighty-nine million miles for the asteroids. It is only in the most distant portion of this space, where small bodies would be less likely to be detected, that none have yet been discovered.*

The foregoing is submitted to your inspection with much diffidence. An author, you know, can hardly be expected to form a proper estimate of his own performance. When it is considered, however, that my formula involves the distances, masses, annual revolutions, and axial rotations, of all the primary planets in the system, I must confess, I find it difficult to resist the conclusion that the law is founded in nature.

Very respectfully, your obedient servant,

DANIEL KIRKWOOD.

Mr. WALKER then remarked, that it would be noticed from the contents of the letter, that Mr. Kirkwood offers for the consideration of the members, a subject of great interest to the cultivators of physical science.

The slightest approach to an analogy, such as constituted any one of Kepler's harmonies, three of which turned out to be his three celebrated laws, should be examined with care, in order that its merits, if it have any, should not be overlooked.

Accordingly, he proceeded at once to verify the numerical data and conclusions of Mr. Kirkwood. He found in them nothing requiring modification, except perhaps the substitution of some more recent values for the masses of Mercury and Uranus. He, therefore, communicated the subject confidentially to the American Philosophical Society, during his stay at Philadelphia, at their stated meeting, July

* It may be proper to remark that *one planet* between Mars and Jupiter, with a mass and mean distance of about double those of the former, would perfectly satisfy the conditions of my theory.

20th; and, in like manner, on the 23d, to Joseph Henry, LL.D., Secretary of the Smithsonian Institution. The next day he replied to Mr. Kirkwood.

The Secretary then read Mr. Walker's letter to Mr. Kirkwood, and the reply of the latter, as follows: —

Washington, July 24, 1849.

To DANIEL KIRKWOOD, Esq., Pottsville, Pa.

Dear Sir: — Absence from the city, and pressing business, have prevented me from sooner answering your esteemed favor of the 4th.

I have tested your theorem, with the most recent values of the masses of the primary planets, namely: those used by Encke, in 1845, with the exception of Uranus, for which I used Adams's recent value, $\frac{1}{21600}$.

I find your quantity $\frac{n}{D^{\frac{3}{2}}}$ to be nearly constant for the primary planets Venus, the Earth, and Saturn. Such a coincidence is hardly attributable to chance, and seems to indicate a law of nature.

Doubtless, in your researches on the subject, your attention has been turned to La Place's nebular hypothesis, from which one of your elements, namely, (D,) is derived.

I think that the form of your constant may be readily deduced from the nebular hypothesis, in which case both theories will strengthen each other.

The subject is highly interesting to men of science, and I shall be glad to make your discovery public, through the medium of some of our journals and scientific memoirs. Please let me hear from you at an early date.

Yours truly and respectfully,
SEARS C. WALKER.

—

Pottsville Academy, July 31, 1849.

SEARS C. WALKER, Esq., Washington, D. C.

Dear Sir: — Your favor of the 24th inst. was received in due time; and I can assure you I have read it with more than an ordinary degree of interest.

My attention was first turned to the subject of the planets' rotations about ten years since. How many hypotheses I have formed, tested, and rejected, and how many hours of solitary toil the subject has cost

me, I shall not undertake to tell you. The theorem which I sent you had been lying on my table since August, 1848, and was, as you have supposed, derived from the nebular hypothesis. A few months since, I sent it to Mr. E. C. Herrick, of New Haven. He declined, however, expressing any opinion on the subject, and suggested that I should submit it to yourself. . . . . . .

I feel under great obligations to you for your offer to make what you are pleased to call my "discovery," public, "through the medium of some of our journals and scientific memoirs." I regret that I have nothing in proper form for publication; and, as I am about leaving Pottsville, to spend my August vacation in the country, I shall not be able to give the subject attention before September. . . .

Allow me to leave the matter entirely in your hands. Any thing I have written is at your disposal. Should you see proper to publish an article, in your own name, giving me credit for suggesting the law itself, I should be highly gratified.

The mass of Uranus, $\frac{1}{31600}$, which you have used, is one of which I had not heard. Struve's value, mentioned in my former letter, was furnished me by Mr. Herrick. I labor under considerable disadvantage from the want of libraries and scientific journals.

I shall be glad to hear from you at any time.

Very respectfully, your obedient servant,

DANIEL KIRKWOOD.

Mr. W. then remarked that the members would be struck with the ten years perseverance of Mr. Kirkwood, in carrying out this leading idea of his life, — with his patience in waiting a year for an opportunity of making known the result, — and with his modesty, (the characteristic of genuine worth,) in offering to the world, the most important harmony in the Solar System discovered since the time of Kepler, which, in after times, may place their names, side by side, in honorable association.

At the instance of Mr. Kirkwood, Mr. W. had tested the numerical accuracy of the hypothesis, by the use of the most recent values for the elements of the Solar System. The mode of proceeding was as follows: —

Examination of Kirkwood's Analogy.

In order that Kirkwood's Analogy should apply to all the planets of the Solar System, including the interpolated planet between Mars and

Jupiter, four fundamental conditions must prevail for each planet considered as the middle of a consecutive series of five; namely:

$$\text{I.} \ldots\ldots \quad a = \frac{(a_{,} + b_{,})\, c_{,} + (a' - b')\, c'}{c_{,} + c'}$$

$$\text{II.} \ldots\ldots \quad m = [\,(a - a_{,} - b_{,})c_{,}]^2$$

$$\text{III.} \ldots\ldots \quad m = [\,(a' - a - b')c']^2$$

$$\text{IV.} \ldots\ldots \quad \theta = \frac{2\pi}{\varkappa} . \alpha^{\frac{3}{2}}$$

Where

$a_{,,,}, a_{,}, a, a', a'' =$ the five mean distances of the planets.

$m =$ the mass in parts of the sun's mass similarly accented.

$T =$ the sidereal year, in earth's mean solar days.

$\theta =$ the siderial rotation in the same.

$k =$ Gauss's *revolution constant* from Kepler's third Law.

$\varkappa =$ a similar *rotation constant* from Kirkwood's Analogy.

$r_{,} =$ the inner radius of the sphere of attraction for the third planet.

$r' =$ the outer radius of the sphere of attraction for the third planet.

$r_{,,} =$ the outer radius of the sphere of attraction for the second planet.

$r'' =$ the inner radius of the sphere of attraction for the fourth planet.

$D = r_{,} + r' =$ Kirkwood's diameter of the third planet's sphere of attraction.

$$\alpha = \frac{a}{D}$$

From these definitions the value of D is thus derived:

$$\frac{m_{,}}{r_{,,}^2} = \frac{m}{r_{,}^2}$$

$$\frac{m}{r'^2} = \frac{m'}{r''^2}$$

$$r_{,,} = a - a_{,} - r_{,}$$

$$r'' = a' - a - r'$$

$$\frac{m}{r_{,}^2} = \frac{m_{,}}{(a - a_{,} - r_{,})}$$

$$\frac{m}{r'^2} = \frac{m'}{(a' - a - r')}$$

$$r_{,} \sqrt{m_{,}} = (a - a_{,} - r_{,}) \sqrt{m}$$
$$r' \sqrt{m'} = (a' - a - r') \sqrt{m}$$
$$r_{,} = \sqrt{m} \left(\frac{a - a_{,}}{\sqrt{m} + \sqrt{m_{,}}} \right)$$
$$r' = \sqrt{m} \left(\frac{a' - a}{\sqrt{m'} + \sqrt{m}} \right)$$
$$D = (r_{,} + r')$$
$$= \sqrt{m} \left(\frac{a - a_{,}}{\sqrt{m} + \sqrt{m_{,}}} + \frac{a' - a}{\sqrt{m'} + \sqrt{m}} \right)$$
$$= \left(\frac{2\pi}{\varkappa} \right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta} \right)^{\frac{2}{3}} \cdot a$$
$$b_{,} = \left(\frac{2\pi}{\varkappa} \right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta_{,}} \right)^{\frac{2}{3}} \cdot a_{,} - \sqrt{m_{,}} \left(\frac{a_{,} - a_{,,}}{\sqrt{m_{,}} + \sqrt{m_{,,}}} \right)$$
$$b' = \left(\frac{2\pi}{\varkappa} \right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta'} \right)^{\frac{2}{3}} \cdot a' - \sqrt{m'} \left(\frac{a'' - a'}{\sqrt{m''} + \sqrt{m'}} \right)$$
$$c_{,} = \frac{\sqrt{m_{,}}}{b_{,}}$$
$$c' = \frac{\sqrt{m'}}{b'}$$
$$b_{,} = \sqrt{m_{,}} \left(\frac{a - a_{,}}{\sqrt{m} + \sqrt{m_{,}}} \right)$$
$$b' = \sqrt{m'} \left(\frac{a' - a}{\sqrt{m'} + \sqrt{m}} \right)$$
$$\sqrt{m} + \sqrt{m_{,}} = (a - a_{,}) c_{,}$$
$$\sqrt{m'} + \sqrt{m} = (a' - a) c'$$
$$m = [\,(a - a_{,} - b_{,}) c_{,}]^2$$
$$m = [\,(a' - a - b') c']^2$$
$$a = \frac{(a_{,} + b_{,}) c_{,} + (a' - b') c'}{c_{,} + c'}$$

Computation of the Value of the Constant called $\varkappa$.

If we use Leverrier's* mean distances and masses in his theory of Mercury, with the exception of Adam's† value $\frac{1}{21000}$ for the mass

* Additions a la Connoissance des Temps, 1848, 17-26.
† Proceedings R. A. Soc. Vol. IX., pp. 159, 160.

of Uranus, and Hansen's‡ periods of rotation, we find by condition (IV.) the following values of

$$\varkappa = \frac{2\pi}{\theta} . a^{\frac{3}{2}}$$

Namely:

$$\begin{aligned} \varkappa &= 15.179 \text{ by Venus.} \\ &= 14.811 \text{ by Earth.} \\ &= 15.593 \text{ by Saturn.} \end{aligned}$$

Whence, with double weight for Saturn:

$$\varkappa = 15.300 = \text{mean value adopted.}$$

If we form another constant $k' = \left(\frac{\varkappa}{2\pi}\right)^{\frac{2}{3}}$ we find

$$\begin{aligned} \text{By Venus, } k' &= 1.9377 \\ \text{Earth, } \quad &= 1.9054 \\ \text{Saturn, } \quad &= 1.9772 \end{aligned}$$

From which it appears that an approximate value for the rotation times might be obtained from

$$\theta = \left(\frac{a}{2D}\right)^{\frac{3}{2}}$$

But the other formula is preferable; and we have

$$\theta = \frac{2\pi}{\varkappa} . a^{\frac{3}{2}} = \frac{2\pi}{15{\cdot}3} . a^{\frac{3}{2}} = \frac{\pi}{7.65} . a^{\frac{3}{2}}$$

With this value of $\varkappa$, using the data above mentioned, interpolating the a, m, and θ of the fifth or hypothetical planet, called Kirkwood, and three masses, viz.: of Mercury, Mars, and Uranus, the following normal elements of the primary system are obtained, in which all of the above four fundamental conditions are fulfilled for each middle planet of five. For Neptune, Mr. W. had used his own value of the mean distance, and Prof. Peirce's mass from Bond's measures of the elongation of the satellite. The interpolated values are enclosed in parentheses.

‡ Schumacher's Jahrbuch, 1837.

Table of Planetary Elements, conforming rigorously with Kirkwood's Analogy.

Planet.	Mean distance.	Mass in parts of the sun's mass.	Sidereal revolution in earth's mean solar days.	Diameter of the sphere of attraction.
	a	m	θ	D
Mercury* . .	0.387.099	$\left(\frac{\dagger 1}{2802311}\right)$	1.003.473	(0.198.122)
Venus . . .	0.723.333	$\frac{1}{401847}$	0.972.917	0.377.908
Earth . . .	1.000.000	$\frac{1}{\ddagger 354936}$	0.997.270	0.513.934
Mars	1.523.691	$\left(\frac{1}{2107404}\right)$	1.025.936	0.768.429
Kirkwood . .	(2.908.511)	$\left(\frac{1}{1353240}\right)$	(2.406.104)	(0.830.951)
Jupiter . . .	5.202.800	$\frac{1}{1050}$	0.385.907	5.035.373
Saturn . . .	9.538.852	$\frac{1}{\S\ 3512}$	0.437.003	8.497.477
Uranus . . .	19.182.730	$\left(\frac{1}{23733}\right)$	(1.396.779)	(7.875.342)
Neptune . . .	30.039.500	$\frac{1}{20000}$		

* An interpolated planet with a mean distance $a = 0.20$ and a mass of $\frac{1}{4739670}$, would harmonize with the above system.

† This interpolated mass of Mercury compares with observation thus: —

Leverrier's first mass, . . .	$\frac{1}{1.909.700}$
Walker's interpolated mass, .	$\frac{1}{2.802.311}$
Leverrier's second mass . .	$\frac{1}{3.000.000}$
Encke's value, ...	$\frac{1}{4.865.771}$

‡ And for Mars:

Burckhardt's mass, . . .	$= \frac{1}{2.680.637}$
Walker's interpolated do. .	$= \frac{1}{2.107.404}$

§ And for Uranus:

Adam's mass, ...	$\frac{1}{21000}$
Walker's interpolated mass, . .	$\frac{1}{23733}$
Lamont's mass, ...	$\frac{1}{24605}$
Struve's mass, ...	$\frac{1}{26860}$

Conclusions from the above Table.

From this comparison of authorities, it appears that with a constant value of $\varkappa = 15.300$, and with assumed masses of Mercury, Mars, and Uranus, equally plausible with those heretofore employed, the a, m, and θ, of the fifth or hypothetical planet may be thus interpolated:

$$^{*}\, a = 2.908.511$$
$$m = \frac{1}{1.353.240}$$
$$\theta = 2.239.035$$

And then the system of nine values of a, m, and θ, will be normal with reference to Kirkwood's analogy, and each of the four fundamental conditions (I.) (II.) (III.) and (IV.) will be rigorously fulfilled for every middle planet of five.

Mr. Kirkwood had remarked in his first letter, that his analogy required the assumption of a fifth planet between Mars and Jupiter. If the Geological Section was allowed the privilege of restoring fishes, lizards, and elephants, there was no reason why the Physical Section should not be permitted to restore a planet.

Remarks on the Degree of Constancy of $\varkappa$.

The limits within which it is possible to vary the value of $\varkappa$, without making some of the interpolated elements inadmissible, are about one-twentieth of the adopted mean value of 15.300. We may therefore conclude that, WHETHER KIRKWOOD'S ANALOGY IS OR IS NOT THE EXPRESSION OF A PHYSICAL LAW, IT IS AT LEAST THAT OF A PHYSICAL FACT IN THE MECHANISM OF THE UNIVERSE. The quantity D, on which the analogy is based, has such immediate dependence upon the nebular hypothesis, that it lends strength to the latter, and gives new plausibility to the presumption that this, also, is a fact in the past history of the solar system.

Such, then, is the present state of the question. Thirty-six elements of nine planets, (four being hypothetical,) appear to harmonize with Kirkwood's analogy in all the four fundamental equations of condition for each planet.

To suppose that so many independent variable quantities should harmonize together by accident, is a more strained construction of

* The mean distance is greater than that of the asteroids, except Hygeia, which has $a = 3.18$.

the premises than the frank admission that they follow a law of nature.

If, in the course of time, the hypotheses of La Place and Kirkwood shall be found to be laws of nature, they will throw new light on the internal organization of the planets, in their present, and in any more primitive state, through which they may have passed.

For instance, we may compute the distance p, from the centre, at which any planet must have received its projectile force, in order to produce at the same time its double movement of translation and rotation. Now let v = the planet's present angular rotatory velocity. Then, $K = p\ m\ v$, will be a constant quantity denoting its momentum of rotation.

If the planet, in a more primitive state, existed in the form of a ring revolving round the sun, having its present orbit for that of the centre of gravity of the ring, the momentum K of rotation must, by virtue of the principle of conservation of movement, have existed in some form in the ring. It is easy to perceive that this momentum K is precisely the amount which must be distributed among the particles of the ring, in order to preserve to all the condition of dynamical equilibrium, while those of each generating surface of the ring were wheeling round with the same angular velocity. It is also clear that this mean angular velocity must be that of the primary planet in its orbit, and accenting the quantities p and v for the case of the ring, we have the equations,

$$K = pmv = p'\ m\ v'$$

$$\frac{p'}{p} = \frac{v}{v'} = \frac{T}{\theta}\ \frac{\varkappa}{k}\ D^{\frac{3}{2}}$$

$$p' = \frac{\varkappa}{k} . D^{\frac{3}{2}} . p$$

But on the hypothesis that the law of decrease of density from centre to surface in the primitive shape was the same as at present,

Let r = the present radius of the planet;

R = that of the generating figure of the primitive ring;

Then, $\frac{p'}{p} = \frac{R}{r}$

And, $R = \frac{\varkappa}{k} . D^{\frac{3}{2}} . r$

The value of R from this formula, comes out a very small fraction of D for the small planets, and nearly equal to D in the case of Jupiter, Uranus, and Saturn. If any inference can be drawn from this result, it is unfavorable to the hypothesis that the primitive law of decrease of density was the same as the present.

If the planets have really passed from the shape of a revolving ring to their present state, the prevalence of Kirkwood's analogy shows a nice adaptation of parts, in every stage of the transition.

If the primitive quantity of caloric (free and latent) had undergone a very great change beyond that now indicated in the cooling of their crusts; if the primitive quantity of movement of rotation had been different from its actual value for any planet; if the law of elasticity of particles for a given temperature and distance from each other varied from one planet to another in the primitive or present states; in either of these cases, the analogy of Kirkwood might have failed. As it is, no such failure is noticed; we are authorized, therefore, to conclude, that the primitive quantity of caloric, — the law of elasticity, — the quantity of movement of rotation, — the past and present radii of percussion, — the primitive diameter of the generating surface of the rings, and the present dimensions and density of the planets, have been regulated by a general law, which has fulfilled for all of them the four fundamental conditions of Kirkwood's hypothesis.

After Mr. Walker had concluded, Mr. GEORGE P. BOND inquired of Mr. W. as to the applicability of his remarks on primary rings to the case of the secondary ring in the system of Saturn.

Mr. W. replied that in the case of the breaking up of a primary ring, the day of the new planet would be equal to the year of the ring, provided the new diameter was the same as that of the generating figure, and the same law of decrease of density from centre to surface was preserved. In this case we should have

$$K = rmv = r'mv'$$

$$v = v'$$

and therefore, $$p = p'$$

Such, however, is not the case in fact with the primary planets. The new diameter is contracted by the more immediate action of the central mass, more than it is expanded by the increase of free caloric. The new diameter is, therefore, so much smaller than the primitive D, that p' is changed into p, and v', or the yearly mean angular velocity, is changed into v for the daily value.

We may extend the nebular hypothesis, and Kirkwood's analogy to the secondary systems. If they are laws of nature, they must apply to both. In the secondary systems the day and month are the same. This fact has remained hitherto unexplained. Lagrange showed that if these values were once nearly equal, a libration sets in round a state of perfect equality; but he offered no conjecture as to the cause of the primitive equality. On the nebular and Kirkwood's hypothesis, it would only be necessary that upon the breaking up of the ring, the primitive diameter of the generating figure and law of relative density of layers, should be preserved, in order to maintain a constant value of $p = p'$, and consequently of $v = v'$.

Prof. Henry has shown that the moon, and probably the other satellites, by excess of radiation above absorption, have reached their constant minimum amount of free and latent caloric.

Perhaps this is the very condition required to maintain $p = p'$, and consequently $v = v'$. In this case we may conclude that p had exceeded p' immediately after the breaking of the ring, and only arrived at a state of equality by the loss of caloric from radiation.

After this reply of Mr. W., Mr. S. W. Roberts, of Philadelphia, remarked that few subjects had been brought before the Association possessing equal interest for the cultivators of physical science.

Prof. Henry said that he had listened with admiration to this communication. He then made some further remarks on the origin of the primitive state of matter called the nebular hypothesis, and on the gradual diminution of the primitive quantity of caloric from excess of radiation over absorption.

Prof. Peirce remarked that Kirkwood's analogy was the only discovery of the kind since Kepler's time, that approached near to the character of his three physical laws. Bode's law, so called, was at best only an imperfect analogy. Kirkwood's analogy was more comprehensive and more in harmony with the known elements of the system. The diameter of the sphere of attraction, a fundamental element in this analogy, now for the first time gave an appearance of reality to Laplace's nebular hypothesis, which it never had before. The positive testimony in its favor would now outweigh the former negative evidence in the case, however strong it may have been. It follows at least from Kirkwood's analogy, that the planets were dependent upon each other, and therefore connected together in

their origin, whatever may have been the form of the connection, whether that of the nebular hypothesis, or some other not yet imagined.

On the Phenomena attending the Disappearance of Saturn's Rings. By Mr. George P. Bond.

[Not received.]

A New Demonstration of the Parallelogram of Forces. By Prof. Benjamin Peirce.

[Not received.]

A New Demonstration of the Parallelogram of Forces. By Mr. J. D. Oliver.

[Not received.]

These communications were followed by a discussion, in which Professors Bache, Peirce, and Henry, joined.

Adjourned.

B. A. GOULD, Jr., *Secretary*.

Fifth Day, August 18, 1849.

SECTION OF CHEMISTRY, MINERALOGY, AND METEOROLOGY.

J. H. Wurtz was called to the chair, and E. N. Horsford appointed Secretary.

On some Experimental Determinations of the Economic Values of British and American Coals. By Prof. W. R. Johnson.

Prof. Johnson alluded to the former labors of Lavoisier, Rumford, Dulong, Parkes, Manby, Schaufhautl, Fyfe, and others, in Europe, and to those of Bull, Dana, Francis, Hayes, and Stevens, in this country, as instances of the talent and industry which had been applied to the determination of the law of heating power in combustible bodies.

He briefly recapitulated the circumstances which led to the under-

taking of a series of researches on American coals, in 1842, pointed out the necessities of the naval service and the other great interests of the country, which had induced the authorities at Washington to enter into this most important research, and mentioned the extension then given to the inquiry.

When the Report on American Coals reached England, in 1845, a copy was sent by Hon. Joseph Hume, M. P., to the Lords of the Admiralty, with a suggestion that a similar examination should be made of the coals of England, Scotland, and Ireland. Their lordships promptly responded to this wish, and called on Sir Henry de la Beche and Dr. Lyon Playfair, to undertake the investigation. After a labor of two years and a half, employed in testing and analyzing the coals, their report was rendered in January, 1848.

By means of this report on British coals, and of his own on American coals, Prof. Johnson was enabled to institute a series of comparisons not only between the several classes, but in some cases directly between individual samples of the coals of the two countries, having a similar constitution, and apparently the same physical character. The number of samples of fuel tested at Washington, in 1843, was *forty-five*; and the time devoted to the whole investigation, one year and eight months; the number tried at London was *thirty-one*. Of the American trials, *eight* were upon anthracites, *ten* were free-burning, semi-bituminous coals of Pennsylvania and Maryland; *ten* highly bituminous coals of Eastern Virginia; *six* similar bituminous coals of England, Scotland, and Nova Scotia; *two* western bituminous coals; *one* was on "natural coke" of Virginia, *two* on artificial cokes, *two* on mixtures of anthracite and bituminous coal, and *one* on pine wood.

Of the British series, *two* were anthracites, *ten* * were free burning coals of Wales, *eight* highly bituminous coals of Wales, (resembling those of Eastern Virginia,) *two* English, and *five* Scotch coals of high bituminousness; *three* were patent fuels, and *one* was wood, — species not stated.

The evaporating vessels used in the American and in the British researches were different in construction. Prof. J. had used a cylindrical boiler, thirty feet in length, three and a half feet in diameter, having two interior return flues, each one foot in diameter, and side flues exterior to the boiler, by means of which the gases went completely round the boiler, after returning through the interior flues, making

* The Welsh coals compared are, the Graigola, Oldcastle fiery vein, Ward's fiery vein, Binea, Llangenneck, Duffryn, Mynydd Newyd, Resolven, Bedwas, and Ebbw Vale.

the entire length of circuit for the products of combustion, from the centre of the grate to their entrance into the chimney, 121 feet up. The grate surface was 16¼ square feet, the area of heat-absorbing surface was 377½ square feet; so that the ratio of grate surface to absorbing surface was as 1 to 23¼. The chimney was 63 feet high, with a cross section of 324 square inches. The fire was built beneath the boiler.

The British Commissioners used a boiler of the Cornish form, twelve feet long and four feet external diameter, having an interior flue two feet in diameter, within which the fire was built. The products of combustion having traversed this flue, returned in a divided current through side flues exterior to the boiler, and finally in a united current passed under the boiler to the chimney, making a circuit of 36 feet in length. The heat-absorbing surface was 197.6 square feet, the area of grate, five square feet, and the ratio of grate surface to absorbing surface 1 to 39½. The chimney was 35½ feet high, and had a cross section of 182¼ square inches. The amount of coal burned during the whole series of 144 trials at Washington, was 62½ tons; that consumed in the 82 trials at London, was 14⅜ tons; the average weight of coal burned at one trial, in the former case, being 978 pounds, and in the latter, 391½ pounds.

The shape of the Cornish boiler gives rise to an inequality of temperature of the water in its various parts. The actual difference between the surface and the bottom water averaged 70°, (Brit. Rep. p. 6,) and compelled the experimenters to adopt the expedient of pumping the cooler water from the bottom through a series of pipes into the upper part of the boiler, whenever it became necessary to get the mean temperature of water in the boiler.

In both the American and the British trials, the economic weight of all the samples tested, was determined by measuring them, not in bushels, but in cubic feet, and the relation of the economic weight to the specific gravity of the coal, as found in the mine, has been ascertained for each sample. This determination had enabled Prof. J. to give in his tables the cubic space required for the stowage of a gross ton of each kind of coal. He had ascertained this for the coals as received, and in their ordinary marketable condition as to the size of the lumps. The British Commissioners, on the contrary, produced an artificial economic value by breaking every kind of coal up before weighing, into fragments so small that no piece should weigh more than one pound. This treatment caused nearly every

sample to exhibit a higher economic weight than it would have done had it been weighed in the marketable state — that state in which it is usually put on board of steam vessels.

Having determined the weight of a cubic foot of coal by direct experiment, and also the weight of water which a pound of each coal would convert into steam at 212°, Prof. J. had computed and given in his report to the Navy Department, (Senate Document, 386, twenty-eighth Congress, first session,) the weight of water which one cubic foot of each coal would convert into steam from 212°, and had made this the basis of his fifth table of ranks, (Report, p. 594.)

By breaking up their coals to the degree of fineness above-mentioned, and thereby giving them an artificial economic weight, the British Commissioners have obtained, in nearly every case, greater quantities of steam per cubic foot of coal than were given by analogous coals in the American trials. Thus the free-burning coals of Wales, which are analogous to those of Maryland and Pennsylvania, have an average specific gravity of 1.31, while their American congeners have 1.357, or the latter are 3½ per cent. heavier in the mine, than the former; yet the twelve American free-burning coals, weighed in the marketable state, exhibited 52.84 pounds per cubic foot, and the ten Welsh, free-burning coals, 54.45 pounds. Adding to this latter weight 3½ per cent. for the greater specific gravity of the American coals, we have 56.35 pounds, as the weight of one cubic foot of them, if prepared by breaking up, in the way above described. The above weight of 54.45 pounds of British coals gave an average of 543 pounds of steam; and the 52.84 pounds of American coal gave 510.35 pounds of steam. At this rate, 56.35 pounds of the same coal gave 544.2 of steam, showing the economic values, bulk for bulk, of the two coals in that state to be almost identical.

By comparing about twenty different samples of American with the same number of British coals having *corresponding specific gravities*, it is rendered highly probable that by the treatment to which their coals were subjected by the British Commissioners, (that is by breaking them up into fragments so small, that no piece should weigh more than a pound,) any given space is made to receive, on an average, $10\frac{2}{10}$ per cent. more weight than when the same coals are measured in their marketable state, and without this artificial preparation. As a guide to practice these artificial economic weights must prove fallacious, unless the steamships can be brought to the adoption of the same standard for the size of their coals.

It was remarked that though the British Commissioners determined the quantity of moisture in the coals upon which they experimented, they made no account of the results in their computation of heating power. This, Prof. J. regarded as objectionable, especially where the moisture amounted to so great a quantity as that given in some of the analyses. Thus of the two Dalkeith Scotch coals, that which came from the "Jewel seam" contained 9.36 per cent. of moisture, and evaporated only 7.08 pounds of water from the boiler per pound of coal, while that from the "Coronation seam" gave but 5.88 per cent. of moisture, and evaporated 7.71 pounds of water from the boiler. In both cases this hygrometric water replaced so much coal, when weighed out to the fireman, and for *that* reason was to be deducted from the weight, in order to get the actual weight of coal burned. Besides this, as so much water was thrown upon the grate to be evaporated, instead of being put into the boiler for that purpose, for *this* reason also it must be regarded as having been deducted from the *useful* effect of the fuel. Consequently, the weight of coal must in each case be reduced by the per centage of its moisture, and the weight of water considered as delivered to the boiler, must be increased by a like per centage of the weight of coal burned, to get the relative values of the two fuels in like states of dryness. If we deduct the weight of ash in each of these two coals, from 100, we get the combustible, including moisture,=95.63 and 96.90; and computing the steam for one pound of this combustible matter, we get 7.40 and 7.95, of which the difference is 0.55 pound, or 7.4 per cent. of the smaller number. If again we deduct the per centage of moisture in each coal, from that of the combustible, we obtain 86.27 and 91.02 as the true relative quantities of dry combustible in each variety, and adding to the weight of water evaporated from the boiler by each pound of the moist combustible, the weight of moisture which it evaporated from the body of the coal itself, we obtain 7.17 and 7.76 as the respective amounts evaporated per pound of moist coal; and as these quantities were evaporated by .86 and .91 of a pound of *dry combustible matter*, we obtain as the evaporative efficiency of one pound of such combustible, 8.37 and 8.53, or the difference is reduced to $\frac{22}{100}$ of a pound of water to each pound of combustible, which is 2.6 per cent. only of the smaller number. This difference may be accounted for by the difference in the composition of the dry combustible of the two varieties of coal. In the Jewel seam, the fixed carbon was to the volatile combustible as 1.11 to 1, while in the

Coronation seam it was as 1.24 to 1. This greater evaporative efficiency among bituminous coals, in proportion as the ratio of their fixed to their volatile combustible material is higher, is a general truth, established as well by the British as by the American experiments. It was fully brought out in the report on American coals. The comparison of any number of the bituminous coals differing essentially from each other in the amount of their volatile constituents, will, whether drawn from the American or the British tables, be found to confirm this general conclusion.

Thus the ratios of fixed to volatile combustible matter, and the evaporative power of the whole of the combustible matter, will be seen by the following comparisons, of which the first is from the American, and the second from the British trials.

American Experiments.

	Ratio of fixed to volatile matter.	Evaporative power.
1. Scotch Coal, ...	1.257	7.72
2. Newcastle Coal, ...	1.600	9.18
3. Virginia Midlothian, (new shaft,)	1.680	9.75
4. Cumberland, (Atkinson & Templeman,)	4.940	11.62

British Experiments.

1. Scotch, (Dalkeith, Jewel seam,) .	1.112	7.42
2. Broom Hill, (not far from Newcastle,)	1.780	8.96
3. Cwmfrwd Rock Vein, (Welch,) .	2.08	9.07
4. Ebbw Vale, (Welch,) . .	3.59	10.52

The same point might be illustrated by numerous other examples from both reports.

Prof. J. noticed with approbation the determination by the British Commissioners of the relative cohesive powers of the several coals, or their power to bear transportation; also their experiments and computations to ascertain the quantity of ammonia, and of its sulphate, which would be yielded by the destructive distillation of each coal. He recurred to the fact that the American experiments had remained to the present time incomplete, for want of the appropriations necessary for carrying them on, only a part of the coals having been submitted to ultimate analysis, and to a determination of their sulphur, while the British experimenters had been enabled to execute complete series of ultimate analyses, ascertain the proportion of sulphur, and give the proportions of carbon, hydrogen, nitrogen, oxygen, sulphur, moisture, and ash, for every sample of coal. He men-

tioned, as highly interesting to steam navigation, the trials they have made of several patent fuels, by which they proved that some of those fuels contain a much greater evaporative power under a given bulk than any of the coals in their ordinary merchantable state. This was illustrated by a comparison between Warlich's patent fuel, of which

	lbs. of steam.
One cubic foot gave	715.35
And Ward's Fiery Vein Coal, . .	608.78 (a maximum.)
And Dalkeith's Jewel seam, . .	352.58 (a minimum.)

He also noticed that the coëfficient of Regnault had been adopted by the British Commissioners for computing the latent heat of the vapor of water, and stated the tendency of this coëfficient to bring out a higher calculated calorific efficiency than that which would be given by the coëfficient directly determined by his own researches.

He observed that they had employed a part of the coal burned in giving temperature to the boiler, its contents and the brickwork of its setting, and a part in generating steam, instead of heating up the boiler and the furnace with wood, and then using the whole heating power of the coal to generate steam ; thus complicating considerably the calculations. In computing the effect of that part of the fuel which is used in heating up the water in the boiler, they have not included that expended on the boiler itself, which, from its considerable weight and high specific heat, might reasonably require an allowance. As the experiments on American coals were commenced with furnace, boiler, and contents, all at normal temperature, such an allowance was not required.

Though the British Commissioners have made experiments on the gases of the chimney, they have not used them to ascertain how much of the whole heat developed was expended on those gases. They came to a conclusion in regard to the oxygen remaining unconsumed in the gases, identical with that previously reached by the American researches, viz., that in ordinary steam-boiler furnaces of good construction, the oxygen which has not been consumed is from one fourth to one half of the whole quantity originally in the air.

The temperature compared with the observed bulk and weight of water in a boiler has been reëxamined by Messrs. De la Beche and Playfair, and as far as they go their results confirm essentially those of Prof. J. These analogous results are found at page 13 of the American Report, and page 53 of the British Report. From the

different requirement of their experiments, the British Commissioners extended their temperatures only from 70° to 212°, while the American reached from 66° to 230°.

The expansion of water in the supply tank was examined by Prof. J. between 58 and 90 degrees; by the British Commissioners between 40 and 80 degrees.

The average heating powers computed from their experiments by the British Commissioners agree very closely with the averages for corresponding classes of coals given at Washington. Thus by the

American Experiments.

Ten anthracites gave steam to 1 of coal,	9.56
Eleven Pennsylvania and Maryland free burning coals,	9.68
One Newcastle coal,	8.65
Ten Virginia bituminous coals,	8.48
Four Liverpool and Nova Scotia,	8.18
Three Scotch and Western,	7.49
Mean of 6 averages,	8.66

British Experiments.

Two anthracites gave steam to 1 of coal,	9.65
Ten Welch free burning coals,	9.58
One Broomhill,	8.75
One Forest of Dean,	8.52
Eight Welch bituminous,	8.00
Five Scotch,	7.64
Mean of 6 averages,	8.69

The pine wood used at Washington gave 4.69; the wood, of inferior quality, used at London, gave 3.10.

One of the great purposes of researches, such as are now under consideration, is to afford information, not merely with respect to particular samples of coal tested, but on the general laws of calorific efficiency, whereby we may be enabled to establish tests of easy application, in place of the laborious operations involved in the production of steam, on a large scale. It was for this purpose that in the American experiments several ultimate analyses had been performed, the general results of which are found at page 586 of the American Report; and for a like purpose were made all the experiments on burning coals with litharge, (Report, p. 585,) and

those on the composition and character of the gases passing to the chimney. (pp. 561 – 581.) In order to make a strict comparison of the total calorific efficiencies of different fuels, we must know what part of their heating power they respectively expend on the evaporating vessel, and what part on the gaseous products of combustion,— it being well ascertained, that coals of different constitutions, are liable to vary considerably from each other, in the proportion of their effective or useful, and their ineffective expenditures of heat. By a comparison among six different bituminous coals of the computed heating power of their *carbon* alone, as ascertained by ultimate analysis, with their practical heating power, as applied to both the boiler and the products of combustion, the most exact and striking conformity had been found between their per centage of carbon and their relative calorific efficiencies. The computations, for which the data are found in the American Report, have been published in the Proceedings of the Academy of Natural Sciences of Philadelphia, (Vol. II. p. 204.) As the British Commissioners have given ultimate analyses of all their coals, it is practicable to compare the carbon constituent of every sample with its heating power, as expended on the boiler, but not on the gases of combustion. The proofs of this law, furnished by the American and British experiments respectively, are as follows:

American Experiments.

Name of coal.	Theoretical evaporative power of the carbon alone, by Dulong's coefficient = 12.906; and latent heat = 1030°. (A.)	Practical Effects. Water from 212°, evaporated from the boiler by one of dry combustible matter.	Practical Effects. Evaporative power expended on the products of combustion, in steam, from 212°.	Total efficiency of the combustible matter, by experiment. (B.)	Difference between the theoretical and experimental efficiency, from columns A and B.
Cambria County, Pa.	11.522	10.238	1.312	11.550	— .028
Midlothian, Va.	11.731	10.191	1.269	11.460	+ .271
Newcastle, Eng.	10.545	9.178	1.720	10.898	— .353
Clover Hill, Va.	10.445	8.588	1.949	10.537	— .082
Scotch,	10.393	8.868	1.338	10.206	+ .187
Cannelton, Ind.	9.565	7.734	1.823	9.557	+ .008
Average,	10.700	9.133	1.568	10.701	+ .0005

British Experiments.

Coals arranged in the order of their per centage of carbon, beginning with those having the least.	Carbon per cent. by analysis for the set. Mean.	Steam to 1 of coal from 212° by experiment. Mean.	Steam to 1 of coal from 212° by calculation from per centage of carbon.	Difference between experiment and calculation.
1st set of 4 samples	74.15	7.78	8.03	—.25
2d " 4 "	76.63	8.35	8.27	+.08
3d " 4 "	79.67	8.65	8.60	+.05
4th " 5 "	81.06	8.89	8.75	+.14
5th " 4 "	85.68	9.17	9.25	—.08
6th " 4 "	88.12	9.50	9.51	—.01
7th " 4 "	89.99	9.75	9.75	(Standard of comparison.)

All these samples from the British Report were arranged in strict conformity with their carbon constituent, and then merely divided into groups or sets, as stated in the table.

Comparisons had also been made of the practical efficiency of the coals as given by experiments under the steam boiler, with their calculated heating powers as determined both by the Method of Berthier, — that of burning them in contact with litharge, — and by computing from ultimate analysis the sum of the heating powers of their hydrogen and carbon constituents. Neither of these latter methods gives results so nearly in accordance with practice, as that which has its basis in the carbon constituent alone. Still less does that method which makes heating power depend only on *fixed carbon.* Until some proof to the contrary shall have been elicited by future experiment, we must still continue to regard the total amount of *carbon*, both *fixed* and *volatile*, as the true index of calorific efficiency, whether in bituminous coals or anthracites.

In concluding their report the British Commissioners suggest for consideration, that these experiments may be extended to the coals of other districts than those already embraced, and that for this purpose the needful expenditure be sanctioned for one or two years more. Should this be done, they express the belief that "a most important body of information would be accumulated, alike important to the naval service and to the public at large."

A similar statement had been made five years ago in concluding the report then rendered on American coals. At the time that representation was made, the extent and importance of our American coal fields were little understood or appreciated, and the value of direct and careful determinations of their respective heating powers, were

far less highly prized than at present. They are now desired and solicited with great eagerness. Prof. J. pointed out the necessity which still exists for an extensive and thorough research, in order to give the coal fields of the United States, not embraced in the first series of experiments, the same advantages of a close investigation, which had been in part afforded to those east of the Alleghany mountains.

He indicated, as among those which yet await the practical and chemical tests, the coals of western Pennsylvania and Virginia, those of Kentucky and Tennesse, of Ohio, Indiana, Illinois, Iowa, Michigan, and Missouri, of North Carolina, Georgia, Alabama, Arkansas, and Texas.

The navigation of the Gulf of Mexico, and the Caribbean Sea, that of the Northwestern Lakes, in all their extent, and that of the Western Rivers, in all their ramifications, is destined to be conducted almost wholly by the aid of steam generated by coal. Metallurgic operations of vast magnitude will continue to be carried on by the aid of fossil fuel, and no other than the most thorough and exact determination of the relative values of the different sorts of coal, will suffice as an enlightened guide to those who shall hereafter engage either in arts or in navigation.

Prof. J. in conclusion, noticed the fact that the British Commissioners had instituted no comparisons between the results of their own labors and those of others; which might possibly account for their omitting all mention of, or allusion to, the American researches, except by prefixing to their report the letter of Mr. Hume. But he expressed the hope, that though this omission is not without precedent among our scientific brethren on the other side of the water, the cultivators of science in America would not regard it as an example for imitation, and that while pursuing with zeal the career of scientific usefulness, they would be as ready to acknowledge the labors of others, whether at home or abroad, as to claim due credit and consideration for their own.

Leucine and its Homologous Relations, with some Critical Remarks upon the Late Researches of Mr. Wurtz. By T. S. Hunt.

In January, 1848, I published a notice in the American Journal of Science, on the nature of this body, in which I showed that the form-

ula of Mulder, $C_{12}\ H_{12}\ NO_4$, was inadmissible, and suggested that it is a homologue of Glycocoll, and corresponds to the sulphuretted alkaloid of Liebig and Wöhler, named by them Thialdine. I therefore proposed for it the formula $C_{12}\ H_{13}\ NO_4\ HNO_4$. The sarcosine of Liebig is equally a homologue, and these three form as yet the only known members of a group of alkaloids, whose general formula, adopting the above equivalents, is $(C_2\ H_2)^n$.

M. Cahours, in the September following, and more recently MM. Gerhardt and Laurent published papers upon the subject, with numerous analyses of this body and its compounds with nitric and hydrochloric acids, showing the homology already mentioned, and completely establishing the truth of the formula above given; they seem, however, to have been unacquainted with any previous announcement.

M. Laurent had previously shown that Glycocoll was to be regarded as the amid of an acid which is $C_4\ H_4\ O_8$. This acid is the homologue of the carbonic, and sustains the same relation to the acetic acid as that does to the formic. Hence, Glycocoll, as the monamid of a bibasic acid, is itself an acid amid, and capable of exchanging an equivalent of hydrogen for a metal, at the same time that it plays the part of an alkaloid, which double function has been fully established by the researches of Prof. Horsford. The nitrate of Glycocoll is equally monobasic, in accordance with M. Gerhardt's law of saturation.

This new group is isomeric with the amethanes of M. Dumas, Glycocoll with urethylane, sarcosine with urethane, etc., and M. Wurtz, in his researches on the Cyanic Ethers, has recently made known some other new substances which are intimately related to these. I have shown, some time since, that water is to be regarded as homologous with the alcohols, and that consequently *the ethers are homologues of their parent acids*, and M. Wurtz has found that as cyanic acid yields with Ammonia formic urea, the cyanic ethers of wood and wine alcohols, by a similar reaction, give rise to the ureas of the acetic and metacetic series. With water, these ethers are decomposed in a peculiar manner; two equivalents reacting with two of water, evolve two of carbonic acid gas and new crystalline substances, which are identical in composition with the ureas of the acetic and valerianic families and are their isomeres, probably related to them as the Glycocolls are to the amethanes. The Glycocolls being monamids, the new bodies of Wurtz are equally the binamids of their peculiar acids.

The further investigations of Wurtz show, that while Urea, in the presence of a solution of Potash, is decomposed into a carbonate and two equivalents of Ammonia, the acetic and metacetic ureas give rise to one equivalent of Ammonia, and one of a new alkaloid; these are respectively $C_2\ H_5\ N$ and $C_4\ H_7\ N$, and are consequently the homologues of Ammonia.

There are reasons for supposing that while the ureas are double amids of Ammonia and its new homologues with carbonic acids, the isomeres of Wurtz are double amids of two equivalents of Ammonia, with their corresponding acids, and in the same manner the amethanes are probably the amids of the higher alkaloids, while the Glycocolls, which yield only Ammonia, by the action of Potash, confirm the character already ascribed to them. Thus regarded, these otherwise so perplexing isomerisms present no longer difficulties, and appear related precisely as the acetic methylic and formic ethylic ethers. It follows, also, that there cannot exist a homologue of urea in the first family, where the two groups are confounded, and farther, that as we rise in the organic scale, by the existence of new alkaloids isomeric with Ammonia, the number of possible isomeres in these groups is greatly increased.

This paper gave rise to an animated discussion, in which Prof. HORSFORD, Dr. LINCK, Dr. BACHE, and Prof. J. B. ROGERS took part.

ON THE MINERAL WATERS OF CANADA. BY T. S. HUNT.

[Not received.]

NOTE ON SODA IN THE ASHES OF ANTHRACITE COAL. BY PROF. E. N. HORSFORD.

IT has been suggested, in a paper read before the American Academy, and subsequently published in Silliman's Journal, by Professors W. B. and R. E. Rogers, that the fixed alkali, probably present in fossil coal, is volatilized by the high heat of combustion; thus accounting for the circumstance that, in many analyses of coal ashes, no fixed alkali has been recognized. To ascertain whether any potassa or soda might be present, was a question proposed in the investigation of Mr. C. G. Kendall, of Boston, a pupil of the Cambridge Laboratory.

He examined the ashes of the Lehigh coal employed in heating the

Laboratory by steam, and which must have been subjected to as high a heat as ordinarily occurs in the combustion of hard coal.

Determinations of all the ingredients, some of which were several times repeated, were made. No potassa was present. The soda was sought in the aqueous extract, obtained by long digestion with water, and amounted to a *little less than a half per cent.* This quantity, though small compared with the other ingredients of the ashes, is still large when compared with the amount ordinarily present, in soluble condition, in a natural soil; and thus accounts for the fertility which such ashes have sometimes been observed to confer upon soils with which they were mixed. It suggests the propriety of saving such ashes for agricultural purposes, on account of the more soluble alkali, as well as of the less soluble alkaline silicates, which must be more or less present in all fossil fuel.

The ashes of fuel consumed in an open grate, where the heat is lower, would probably contain more soda than is stated above, and probably potassa in addition. Indeed, my friend, Prof. Silliman, Jr., remarked to me, a day or two since, of having found both alkalies present in hard coal ashes, in an investigation still in progress in his laboratory.

Note on the Color of Fused Sulphur. By Prof. E. N. Horsford.

Gmélin has remarked, that the brownness of fused sulphur may be ascribed to a small quantity of asphaltum, accidentally introduced in the manufacture; and adds, that the fact of the slow return to yellowness, as the sulphur cools, is no proof that this explanation is not valid, since the opacity which comes on at the same time prevents the recognition of the brown color.

To decide this point, it was necessary to have sulphur derived from a source where asphaltum could not be present. To this end, ground native sulphate of baryta, mixed with one quarter finely pulverized charcoal, and wet, with meal, to a paste, was reduced in a furnace, in the usual manner. The mass, after cooling and pulverizing, was lixiviated, and the solution exposed to the air for several weeks, in bottles partly filled and loosely stoppered. Upon adding hydrochloric acid to the yellow solution, a dense nearly white precipitate followed, which was gathered upon a plain filter, and washed through three days with distilled water. The white mass upon the filter was then dried, and the sulphur driven off by sublimation.

After subjecting the sulphur to repeated sublimations, it was found to present, upon fusion, the brown color observed when the experiment is made with ordinary sulphur.

On Ammonio-Chloride of Magnesium. By Dr. Christian Linck, Assistant in the Cambridge Laboratory.

In the analysis of compounds of Magnesium, we constantly meet with and apply the fact that there exists a compound of chloride of magnesium with chloride of ammonium, the solution of which is not decomposed by ammonia, or carbonate of ammonia, nor the carbonates of potassa or soda in the cold, nor by many other reagents which act upon pure chloride of magnesium.

The study of this compound has long been neglected. It is, however, so frequently met with by analysts and by students, even at the very beginning of their practical studies, that I considered its isolation and a knowledge of its composition desirable.

These considerations induced me to make an investigation on the subject, the results of which I beg leave to present to this meeting, in the following three sections:

First section.—To a solution of pure chloride of magnesium, water of ammonia was added as long as any precipitate ensued, and the liquid filtered off. Part of this liquid was evaporated to a state of proper concentration, and then crystallized; another part was exposed to the open air in a flat dish for several days, until all odor caused by an excess of ammonia had disappeared. The liquid then re-acted perfectly neutral with test paper. From this liquid the ammonia was determined by precipitation with perchloride of platinum; the magnesia simply by evaporating the solution with an excess of nitric acid, and igniting the residue.

5.221 grms. of the liquid yielded ammonio-chloride of platinum 0.460 = 0.0351 ammonia, or 0.67 per cent.

11,927 grms. of the liquid yielded: magnesia = 0.271 grms. = 2.27 per cent. This shows that for every equivalent of ammonia there are in the solution three equivalents of magnesia.

A calculation from the equivalents of those bodies requires that for every 2.27 per cents. of magnesia 0.62 per cent. of ammonia should be present, a requirement with which my results agree well.

In the way before mentioned two crops of crystals were obtained,

both having the same appearance, and also, as shown by analysis, the same composition. Both were perfectly neutral, colorless, and very deliquescent, so that an accurate determination of the water which they contained was not easily attainable. In the water-bath they lost an amount of water varying in different samples between three and four and a quarter per cents, but without undergoing any striking change of appearance, so that I must conclude that this water is merely adherent to and not combined with the substance. The analysis of this salt, and of all the salts to be described hereafter, refer to a substance which has been dried in the water-bath.

First crop: 1.433 grms, salt yielded, magnesia, = 0.236 grms. or 16.47 per cent. 0.424 grms. salt yielded ammonio-chloride of platinum 0.292 grms. = 0.02228 = 5.25 per cent. of ammonia.

Second crop: 0.996 grms. salt yielded, magnesia, 0.165 grms. = 16.56 per cent. 0.373 grms. salt yielded, ammonio-chloride of platinum = 247 grms. = 0.01884 grms. = 5.05 per cent. of ammonia.

A small calculation will show that magnesium and ammonium are contained in this salt in the proportion of three equivalents of magnesia to one equivalent of ammonia the same proportion as in the liquid from which they were derived. This supposition would require a proportion of 16.5 magnesia to 4.41 ammonia.

These experiments led me to the conclusion that when a solution of chloride of magnesium is decomposed by ammonia at ordinary temperatures, one quarter of the magnesia is thrown down and ammonia takes its place. A compound of one equivalent of chloride of ammonium, with three equivalents of chloride of magnesium, is found in the liquid, and can be crystallized out in combination with a great quantity of water of crystallization.

Second Section. — I now proceeded to ascertain what takes place when a solution of chloride of magnesium is precipitated with ammonia while boiling hot.

The liquid obtained in this manner was investigated in the same manner as the foregoing; part was crystallized, part merely freed from excess of ammonia by standing in the air until it became perfectly neutral. 10,906 grms. of solution gave 0.140 magnesia = 1.28 per cent. 0.876 ammonio-chloride of platinum = 0.067 84 grms. ammonia = 1.11 per cent.

These numbers indicate that the liquid contained chloride of magnesium and chloride of ammonium in the ratio of an equal number of equivalents. 1.28 magnesia, according to this supposition, corres-

pond to 1.07 ammonia. The liquid yielded two different crops of crystals.

The first were mostly very small, and appeared to me to be not all alike, but a mixture of two different salts. The second crop had the appearance of the crystals obtained by the first experiments, mentioned before.

First crop: 1.028 grms. salt yielded 0.111 grms. = 10.79 per cent. magnesia. 0.0445 grms. salt yielding 0.235 ammonio-chloride of platinum = 14.84 per cent. ammonia. This salt, although its composition agrees pretty well with a simple formula ($3NH_4$ C l + 2Mg C l,) I deem to be a mixture, as stated already, and experiments which I shall state below, confirm this opinion.

The second crop of crystals obtained from the liquid under examination, gave the following results: 0.929 grms. salt yielded 0.150 magnesia = 16.13 per cent. 0.343 grms. salt yielded 0.212 ammonio-chloride of plantinum = 0.1604 grms. or 4.68 per cent. of ammonia.

As the result of these experiments, it appears — 1st, that at a boiling heat ammonia precipitates from a neutral solution of chloride magnesium exactly one-half of its magnesia; that is, twice as much as it precipitates in the cold. I may remark here that Pfaff, a German chemist of the last century, gives this as the general result of the action of ammonia at all temperatures — a statement which I feel compelled to contradict.

The second result will appear more strikingly in the next section, and I shall there mention it.

Third Section. — In a solution of chloride of magnesium of known strength, its equivalent of chloride of ammonium was dissolved, and the solution crystallized. The first crop of crystals appeared, on examination, to consist of chloride of ammonium, with which a small quantity of the next salt was mechanically mixed, for the mixture yielded less than one per cent. of magnesia.

The second crop of crystals consisted of the salt 3M g C l, N H_4 C l, described above.

1.182 grms. of the salt yielded magnesia = 0.192 = 16.24 per cent.; 0.332 grms. of the salt yielded ammonio-chloride of platinum = 0.266 = 5.20 per cent. N H_3.

Another solution was made containing one equivalent of chloride of ammonium to three equivalents of chloride of magnesium, and set aside to crystallize. It readily gave large crops of the crystallized compound.

0.430 grms. salt yielded ammonio-chloride of platinum = 0.269, corresponding to ammonia 0.0254 grms. or 4.78 per cent. = 5.11 per cent. NH_4.

1.578 grms. or salt yielding 0.264 Mg O = 16.10 per cent. Mg O = 9.90 Mg.

The salt being neutral, these two data are sufficient to determine its composition. It agrees well enough with the formula $3MgCl, NH_4Cl + 20 Aq$. The composition, according to this formula, would be:

	Calcutated.	Found.
Magnesium	10.06	9.90
Ammonium	4.81	5.11
Chlorine	37.49	
Water	47.63	
	99.99	

The final results of the whole investigation may be laid down in the following propositions:

1. There exists but one double salt of chloride of magnesium, with chloride of ammonium, $NH_4Cl, 3MgCl + 20 Aq$.

2. When, to a solution of chloride of magnesium, ammonia is added in excess, one third only of the magnesium is thrown down, or what amounts to the same thing, in a solution of three equivalents of chloride of magnesium, to which one equivalent of chloride of ammonium has been added, no precipitate will ensue. This liquid yields directly crystals of the ammonio-chloride of magnesium.

3. When the reaction takes place at 212° F. one half of the magnesium is thrown down, but the liquid thus obtained does not yield a corresponding crystallized compound. It decomposes into chloride of ammonium and the double salt of chloride of ammonium with ter-chloride of magnesium. The solution of the latter salt is not acted upon by ammonia in the cold, but at a boiling temperature ammonia drives out part of the magnesia, as must be expected when the experiments adduced in the second section, are taken into consideration.

Adjourned.

E. N. HORSFORD, *Secretary.*

Fifth Day, August 18, 1849.

Morning Session.

SECTION OF GEOLOGY AND PALÆONTOLOGY.

Dr. CHARLES T. JACKSON was appointed to the Chair, and J. B. FELTON, Esq., was appointed Secretary.

The first communication was as follows: —

ON THE ORIGIN OF THE DRIFT, AND OF THE LAKE AND RIVER TERRACES OF THE UNITED STATES AND EUROPE, WITH AN EXAMINATION OF THE LAWS OF AQUEOUS ACTION CONNECTED WITH THE INQUIRY. BY PROF. HENRY D. ROGERS.

Prof. ROGERS expressed his regret that the subdivision adopted this morning of the two very intimately connected subjects of Zoölogy and Geology, should have withdrawn, in their interest in zoölogical topics, some members of the General Section of Natural History and Geology, who had been among the most earnest and successful students of these phenomena, who might differ from himself in many doctrinal points, and who would be glad of an opportunity to reply to his views.

He then proceeded to say, that the interesting problem of the origin of the Drift, and of the other superficial phenomena of transport, of late years the source of so much discussion and the cause of so much research, has scarcely received even yet that clear and critical analysis which its complexity requires, and to which, as involving the consideration of so many questions in Terrestrial Physics, it seems to be obviously entitled. In proceeding to offer certain views of my own respecting this extensive inquiry, I deem it indispensable to a logical limitation of this question, to attempt a preliminary investigation of a few fundamental points involved in the various hypotheses of the Drift, now advocated. To the singular neglect of these essentially antecedent questions, I am disposed to impute much of the remarkable discrepancy discernible in the prevailing doctrines concerning this branch of Geology.

Before entering upon the inquiry of the agency manifest in the Drift, I would proceed to enumerate the various theories of the principal geologists upon this point.

I. The Glacial Theory, based upon the remarkable phenomena of

a slow transport of rocky materials upon the surface of the glaciers, and assuming that the same agency had been employed in the distribution of erratic boulders over the extensive plains of Northern Europe, and over a large portion of North America. The Glacialists suppose that they can discern phenomena incompatible with the agency of water, and identical with those seen to result from the action of the glaciers at this very day, in the very character of these drifted materials. If now, a test, or a series of tests, could be established, by which could be discerned what water can do, and what it cannot do — what ice can do, and cannot do — then might the problem be solved. Before discussing details, let the tests be established.

II. The Iceberg Theory, assuming that the land was submerged when these erratic boulders were diffused. This theory, as advocated by Sir Charles Lyell, assumes that the whole northern part of the continents of Asia and Europe, and North America, were, at that time, beneath the ocean; that the icebergs from the islands broke away — as they now break away from the sloping valleys of Spitsbergen — and, floating along, charged with angular matter, produced the marks, where they rubbed upon the surface, imputed by the Glacialists to the glaciers, and, in melting, as they floated towards the south, deposited these Drift materials as we see them. The first and anterior question would be, whether, at this period, the land was, or could have been, submerged; whether we have any independent proof that these regions were, really, at that epoch, below the sea.

III. Murchison's Theory, assuming the submersion of the land, during the epoch of the Drift, employs the agency of sudden elevation, and waves of translation, for the dispersion of the general drift; and introduces the dispersion of the glacial ice of those regions, by the same sudden paroxymal movements. In this way, he proposes to explain how it is that the granite blocks, and other huge erratics of the mountains of Norway and Sweden, have been strewn across the low plains of Russia, eastward towards the Ural Chain, and southeastward to a distance of 800 miles, up hill, from the granitic region of Scandinavia, gradually ascending to the height of even 1100 feet.

IV. Mr. Robert Chambers's Theory, adopting a similar hypothesis of the submarine condition of the land, conceives, not that the land emerged, but that the sea retired from the surface of Great Britain and Northern Europe, and the northern parts of North America. He seems to take it for granted that the mere fact of the presence of the Drift is, itself, a sufficient proof of submarine deposition. As the

Drift occurs upon the mountains of Scotland, 1500 feet above the sea, and near Snowden, in Wales, at an elevation of 1700 feet, he assumes the ocean to have rested at this height. Upon this view, the land in North America stood 6000 feet lower than at present, leaving only the peak of Mt. Washington as a minute island, standing but a few hundred feet above the waters.

Chambers thinks that he has evidence of successive conditions of repose of the land, at different elevations, in the terraces which are found every where around the shores of Great Britain, and most of the coasts of Europe, at various heights. A careful examination of the proofs, convinces me that the lower terraces, near the sea, are true ancient beaches, possessing the features of genuine sea-margins; but Mr. Chambers is not careful enough in his discrimination of the real tests of watery action; he mounts step by step, passing from the lower terraces, where marine relics exist, to others where they are not to be found,—where the horizontality becomes more uncertain, and even where there is a visible slope, and there is not the slightest trace of erosion, by the surf or other beach action. He himself admits that he loses all traces of marine relics at the height of 360 feet; and I think the facts will show that they cease at even a less height—probably 200 feet. The genuine sea-terraces are the only evidence of the successive levels of the sea, and do not indicate the whole uplifting action to have exceeded 300 or 400 feet at the outside; and afford no proof at all of the general submersion of the northern regions, as maintained in these three hypotheses.

V. The Diluvial Hypothesis, as modified by myself, assumes, not the submersion of the land, but a series of violent elevatory earthquake movements, in the Arctic regions, displacing the Arctic waters, with the ice which bound them, and sending these Southward, across the northern districts of the continents. It supposes the sudden uplifting action to have originated, like those of more modern earthquakes, in different centres and zones of movement, and admits that one of these might have been in the mountains of Scandinavia. Contemplating the vast power and size of the waves of translation, which earthquake undulations in the earth's crust are known to engender, and applying the rigorous calculations of Prof. Hopkins, of England, upon the force of translating waves in propelling large erratics, I see no difficulty in proving that the whole of the Drift could have been dispersed over the area it occupies, with-

out the violent assumption that this area was beneath the sea. My own theory does not reject the auxiliary agency of ice.

Upon a careful study of the phenomena of earthquake action, I have convinced myself that all these movements result in a permanent change of relative level of the land and sea; and that every earthquake consists of an undulating movement of the crust.

Having clearly established these facts, the theory appeals to the agency of the earthquake, as establishing a movement in the waters vastly more energetic than the mere running off of the water from an uprising district, — for it shows that the undulations of the crust below, transmitted to the waters above, must set in motion a series of enormous waves, the transporting power of which we have now the data for estimating.

1. The inquiry which naturally ranks first in the discussion of the Origin of the Drift, River Terraces, &c., regards the character of the medium or agent which transported the fragmentary materials. Was glacial ice the motive power, or was it water, and are there any *physical tests* discoverable in the condition and phenomena of the Drift, and the regions covered by it, any general laws in the earth's geological history, which can assist us to a decision.

2. A second preliminary question respects the signs by which we may ascertain whether a given area of the surface was at a particular epoch dry land, or was covered by water.

These points succinctly treated of, I shall advance in my inquiry into the conditions under which the Drift and our River and other Terraces were formed, by discussing the functions and powers of wide-spread diluvial waves. In the appeal which I shall make, to the modes of action of water, I shall carefully refrain from what might be deemed mere supposition, and confine myself to the known facts and laws of aqueous dynamics, and to a strictly scientific view of the conditions of propulsion and deposition of fragmentary matter, as driven or carried by a fluid.

In venturing to enter upon the inquiry which I have here placed first, whether glacial ice or water, was the transporting agent, and whether we are in possession of any distinctive physical tests by which we can decide, I by no means intend to range over the extensive field of irregular discussion traversed by the advocates of these two rival agencies. My sole object at present is to call attention to a few facts connected with the Drift, which I think must sooner or later be adjudged to possess this important character of decisive tests.

Looking at the wholly different laws of progression of ice compared with those of water, we might, one would suppose, infer, without hesitation, that there must be an essentially different internal structural arrangement in the masses of fragmentary materials driven forward by the two agents. Ice is a solid body, or if slightly plastic when in the form of the glacier, it rests in great volume on a sloping surface, and is acted on by gravity, yet it is essentially a solid body, in its mechanical action on the materials with which it comes in contact. In its propelling action, the glacier has the functions of a wedge or a plough, crushing and grinding the loose fragments which fall upon its surface, between its edges and their enclosing mountain walls, or under its irresistible advancing front, and pushes before it a rude mound or moraine, in which we can discern no marks of a genuine stratification. During its retreating phase, the masses which it still receives, advance just as they do during its increasing stage, to its lower termination, where they are left by the shrinking or retrocession of the ice, on the bed of the valley, at successively higher and higher points, forming one great mound or sheet, as the case may be, of mingled detritus, tumbled together, without any order, or the least approach to a distribution in layers or beds. This absence of stratification, is, I believe, admitted to be a feature of all the moraines in the Alps which have not been worked over and reshaped by the action of water.

Water, on the other hand, by virtue of its mechanical qualities as a fluid, and of its buoyancy, transports its sedimentary burdens in a manner essentially different; according to the size and density of the masses and particles it conveys, it acts with a different propelling energy and a different floating power. When it moves with a flow which is not *too* rapid, or too irregular and tumultuous, it *sorts* and strews or *stratifies*, either minutely or on an extended scale, whatever it suspends or drives along its bed. It derives this sorting or arranging power from the law of its own internal motion, every extended flow or even lesser current, moving in its different parts with very different velocities. Each special subordinate stratum of the fluid, by virtue of this difference of velocity, deposites a different class of fragments, fine or coarse, in accordance with the limits which its own speed affixes to its propelling power, modified a little by the shape and density of the fragments. Ordinarily, or where the current is diffused and sufficiently deep, and maintains an equable flow, there is an increasing gradation of velocity from the bottom and confining

boundaries toward the top and middle, and where the lower part of the stream is loaded with an half rolling and half floating mass of drift material, this gradation of velocity from the rapid diminution of the friction, will be especially marked. We must hence infer that the sorting power of such a sheet of water, just in the stratum where deposition is preparing and taking place, is particularly great. It is obvious, that the kinds of stratification will be determined by the various conditions of the current, such as the steadiness or irregularity of its velocity, the horizontality or eddying and oblique direction of its isodynamic, or equal moving layers and threads, all consequent on the nature of its resisting floor, and likewise by the evenness or diversity in size of the fragments and particles which it is pushing and floating forward.

In a subsequent part of this essay, I shall enter somewhat more thoroughly into this important inquiry of the manner in which a sheet of water, under several conditions of its motion, propels and deposits its sedimentary load; and I shall then attempt to explain upon acknowledged laws, certain internal features of the Drift, which to many geologists seem anomalous and even incompatible with the idea of its transport by rushing or flowing water. My main purpose at present, is to show that while a confused and tumbled intermixture of the rocky debris, such as we behold in every true moraine, is the invariable mark of glacial action, stratification in some mode, either rude or regular, when the predominant feature, is equally distinctive of the agency of a great aqueous wave. I say when stratification is the predominant feature, for I am well aware that though a glacier cannot stratify the materials which it propels, an energetic and tumultuous watery flood, in sorting and regularly bedding one portion of its dregs, does necessarily under special conditions of its motion, throw down the rest in promiscuous disorder, and leave almost no trace of lamination or of any of the marks of a progressive deposition. This feature of absent or defective stratification, common enough in the lower part of the American Drift in certain neighborhoods, and of frequent occurrence in the *Till* of England and Scotland, will claim prominence in another place, and more full and explicit explanation than at present I can pause to give it. I shall here content myself with requesting the student of aqueous dynamics to reflect that those conditions which are essential to the sorting power of water, do not always exist. He will see that where a violent and stupendous flood either sweeps over a very obstructed and uneven floor, or is

composed, as I myself regard every great geological inundation to have been, of a succession of impetuous waves, with intervening checks, or even momentary pauses, stratification amid the turbulent waters and the wild pitching and wholesale propulsion of the lower portion of the Drift, must be an impossibility. Upon the views which I entertain of the nature of the surface movements, which must of necessity attend every energetic displacement and rushing of the terrestrial waters—movements I mean identical with the rapid, and shattering crust undulations of the modern earthquake, but incomparably more energetic, he will easily comprehend how, during the violence of this tossing, the Drift, loosened and lifted from its floor, and borne forward by a succession of stupendous ocean waves, of the kind termed waves of translation, could not well be spread or sheeted out on its rocky floor as when a more steady and less intermitting *current* moved it, but would, inevitably be driven forward confusedly and bodily, and be bodily and suddenly thrown down again by this heaving action. In this phasis of its transport, therefore, the Drift was not floated or even rolled onward by the waters, but was violently *pushed* in oft repeated successive stages, *wholesale* across the rocky bottom. Upon this view of the mode of advance of one part of the Drift, the great abrading power which it has exercised upon its rocky floor, is at once intelligible. Irresistible, indeed, must have been the momentum with which this thick, ponderous bed of angular fragments and huge blocks grated the surface, as at each successive blow of the gigantic billows, aided by the general current, the whole mass to its base, was rapidly launched, one stage further on its course.

Connected with this inquiry into the tests by which we are to ascertain whether ice or water distributed the drift, the *conditions* of the rocky surface upon which it reposes, are of at least equal conclusiveness with the structure of the mass itself.

It has been frequently urged by the glacialists, that the mere existence of the *scratches* and *furrows* and polished surface of the rocky floor of the Drift, is of itself a decisive test of the action of ice, their argument being, that no fluid, no matter what its velocity, could press the fragmentary matter, even the largest angular erratics, with force enough against the bottom, to cause the extensive abrasion and wear, and the sharp and nearly parallel grooving everywhere to be seen upon the hardest rocks. They conceive the appearances to indicate an enormous vertical pressure, such as in their view only a huge

glacier could exert, and a fixity in the grooving edges only compatible with the notion of their being held by a rigid solid, such as ice.

This view, though at first sight rendered plausible, by the well known and easily observed effects of Alpine glaciers, which do certainly thus score and polish the beds on which they glide, is to my apprehension entirely inconclusive, on the simple ground that it has never been accompanied by a critical and scientific examination of the true force and functions of inundating water propelling a mass of debris. It moreover assumes to contradict what may be incontrovertibly proved, that water in full diluvial action, can produce phenomena precisely analogous to those caused by ice. That water, in fit conditions, has this power, is made evident from the effects of even the comparatively feeble and local floods, produced by the bursting of the low dams confining small lakes and rivers. Mr. Milne, in his able essay on the Parallel Roads of Lochaber, presents us with two such instances of this furrowing action by insignificant debacles. One where a high railway embankment breached by accumulated water, formed a torrent charged with a large quantity of earth and stones, which produced on the solid rocks and on large blocks of stone, lying in the path of the stream, "innumerable scratches and ruts;" the other where a head of water twenty-eight feet high, rushing through the town of Greenock, carried away houses, bore forward a "mass of rock weighing sixteen tons, a distance of thirty or forty yards, mixed masses of stone and even of cast iron, with clay and gravel, and marked the streets and walls with furrows." I could cite similar cases reported to me as witnessed in this country, but not having myself seen the localities, I refrain.

Other evidence of the grooving power of gravel, moved by water, equally, if not more conclusive, is to be seen abundantly on the borders of the Delaware, Susquehanna, and other American rivers in latitudes where the most zealous glacialists would not hesitate to admit no glacier ever to have been. At a distance of more than one hundred miles south of the general southern margin of the Drift, which sparsely covers the uplands and plateaux of Northern Pennsylvania, and at various low elevations near these streams, the fixed rocks are distinctly smoothed and striated, and show surfaces indistinguishable from those that are worn and scratched by ice. The occurrence along all these rivers of gravel, and boulder drift torn from more northern sites, and here restricted to the beds of the valleys, while further north it overspreads the uplands, seems, when taken in con-

nection with the very gentle angle of descent, namely, two or three feet in the mile, to prove most conclusively, that it was water, and not ice, which furrowed the rocks and carried forward the gravel.

In relation to the alleged inability of water to produce the scratched and polished surfaces of the rocky floor of the Drift, I would call attention to the fact, that it is the *momentum* which is the measure of the scratching power. The momentum is the weight multiplied by the velocity. A light body, moving at a vast velocity, will have as much momentum as a very heavy body moving very slowly. What a glacier can do, moving at the rate of 200 feet a year, in scratching the surface of the earth, a sheet of gravel, consisting of small pieces, moving at the rate of a mile in a minute, can readily effect. No natural philosopher can resist this conclusion.

There is a velocity at which liquid particles are effectively solid, and this is the cause of the failure of the attempts to drive steamboats with small wheels and rapid revolutions; the liquid water may have all the unyielding character of a solid. But there is another consideration; — the glacier is no heavier than an equal thickness of gravel itself. Suppose the mountain valley, not filled with a glacier 100 feet in thickness, but with a great bed of angular matter, pressed forward by the waters, with a speed immeasurably greater than the slow creeping motion of the glacier. On the Diluvial hypothesis, the weight of a great sheet of gravel, propelled by water, furnishes all the requisite pressure.

Intimately connected with this fundamental inquiry into the existence of test phenomena for determining whether the drift was transported by glacial ice or water, is the interesting question of the *climate* of the Glacial epoch. Now I think it can be most easily shown from the whole tenor of Palæontological facts, that in undergoing a progressive and very gradual cooling of its various climates, the Earth has at no period felt that general sudden chill, which the hypothesis of an enormously wide extension of the Polar snows and ice and of mountain glaciers, during the so-called glacial epoch, of necessity assumes. While no adequate physical cause has ever been assigned for so great a fancied fall of temperature, and for its subsequent rise, the general evidence from the contemporaneous organic remains proves beyond a doubt, that the important law of progressive cooling experienced no such interruption. The two or three instances cited by Sir Charles Lyell, and other writers, of marine shells referable to a subarctic type being found associated with the drift in latitudes consid-

erably south of their existing abode, and which have furnished, I believe, the chief *independent* argument for the supposed glacial winter, are, some of them, too imperfectly established and are too insulated to contradict the conclusions drawn from all the other fossils involved in the drift, and from the general law. I have the excellent authority of Dr. Gould, of Boston, for stating, that the one or two species, (*Tellina Greenlandica*, &c.,) of supposed arctic shells, met with in the Post-Pleiocene clays and sands bordering the St. Lawrence and Lake Champlain, are now inhabitants of the coast of New England, in latitudes quite as far or further south. Since it has been well established that the ocean in the tranquil period, between the earlier and the later drift, flowed through the great natural depression which unites the Gulf of St. Lawrence with the Bay of New York and which contains the basin of Lake Champlain, we should feel no surprise at finding species of the fossils of the coast of Labrador, or of Greenland even, in the deposits of that wide ancient strait. The excessively cold Labrador current, which, obstructed and nearly spent as it is in these latitudes, even now chills the waters of Massachusetts Bay, seems to have found an uninterrupted flow southwestward through the depression mentioned. And no wonder, then, that these subarctic forms flourished and left their relics there. The few cases of northern shells occurring in the drift in Scotland are, I think, susceptible of explanation upon analogous views of the prevalence of cold, arctic currents, caused by very intelligible conditions in the physical geography of the period.

Another and very essential fundamental inquiry relates to the tests by which we are to ascertain whether any particular district, for example, that of the drift, was at a certain epoch covered permanently by the sea, or whether it was in the condition of dry land; or, in other words, to the proofs by which we establish the *presence* or *absence* of the sea or other masses of water at any assumed period of geological time.

Every intelligent geologist will admit that the mere presence of comminuted mineral matter in the form of strata or laminæ is conclusive evidence of deposition from water, either in motion, or in a state approaching to rest; but all are not equally well satisfied that the general *absence* of such deposits is as convincing a proof of the absence of water. It has been maintained, indeed, that as we cannot show that mechanical or chemical deposition is at this time taking place upon the bed of the mid-ocean, we are not warranted in assum-

ing from the non-existence of all sedimentary matter of a special date in any region, that the ocean of that period did not cover it. But when we reflect that no district of the sea has ever yet been sounded without revealing indications of recent sedimentary deposition over at least the chief part of its bottom, and to what prodigious distances its great systematic currents float the impalpable particles swept into it by the larger rivers, and calculate as we easily can* from the terminal velocity of descent of the finer sediments and the speed of the Oceanic currents, the areas over which precipitation must take place, we cannot reasonably believe that even the most central tracts of the sea's bed are destitute of some covering of fine mud or other comminuted matter.

Sir Charles Lyell has proved that even the existing feeble currents of the ocean can waft the sediments, floating out of the great rivers, to a distance of two thousand miles, before they could descend one fourth of the depth of the present Atlantic. Any one who will watch the tardy rate of precipitation, in certain chemical operations, will be ready to admit this conclusion. Now calculate from that rate, how many days or weeks it would take the materials to go down five thousand feet. Take the data, furnished by Lieut. Maury, of the velocity of the oceanic currents, and you would find that the sediments would cross the Atlantic, if the currents went so far, before all could reach the bottom. I contend, therefore, that wherever the sea has rested, it has inevitably left its own ineffaceable record. Wherever the sea has rested, there must remain such a deposition of marine products and sediments, that afterwards no rush of waters could sweep entirely clean the great ocean floor, or wash away from their resting places the sedimentary materials which that ocean bed received.

I have been a student of the action of the earthquakes, and I have found that some modern earthquakes have excited, in the Atlantic, belts of thirty or forty parallel waves of translation, each billow being twenty or twenty-five miles broad. As to the height of these waves, it is recorded that passengers in a steamer, passing near the coast of St. Domingo, during the earthquake of 1843, could see the hills rising and falling like the back of a creeping snake; so that I do not suppose these undulations could have been less than one-hundred feet high, and were perhaps very much higher than this. You will find, in Johnson's splendid Physical Atlas, the limits of the Lisbon earthquake marked, and they reach to the West India Isles. The earthquake

* See Lyell's Principles, Vol. II., "on the area over which strata may be formed."

itself reached to no such distance, for the shocks were felt but a few hundred miles westward of the coast of Portugal. But ten hours after that earthquake had ceased, and had done all its damage in Europe, thirty-six enormous sea-waves came in and struck the cliffs of Guadaloupe and Antigua, at regular intervals of five minutes. Not that the mighty agency was thirty-six times repeated, but the one displacement of the ocean bed took the form of these thirty-six undulations, which were imparted to the sea, its billows flowing on until they crossed the Atlantic.

Now, if the action of the Gulf Stream, or the currents around Cape Horn, or the Cape of Good Hope, have the power of transporting materials to the very centre of the ocean, how much is this conclusion strengthened when we consider that even the most tranquil regions of the ocean are visited by the great transporting waves engendered by the undulations of earthquakes, and that these mighty billows, which can traverse a wide sea, as they did on the occurrence of the Lisbon earthquake, are not mere oscillations of the water, but contain powerful currents, as rapid at the very bottom as at the surface. If such are the forces which even now deposit the materials of the land upon the remotest parts of the bed of the deep, there is surely no reason to assume that at any former epoch, when these physical agencies were, to say the least, as active, the ocean could have stood over any region without receiving upon its floor some thickness of sediment, parts of which remaining, would testify unequivocally at this day to its ancient presence.

So crowded, moreover, is every portion of the existing sea with appropriate living forms, and so full of fossil relics of the past races are all the ancient strata which had their origin from waters in comparative repose, that it seems impossible, on this consideration, also, that any ancient surface, that for example which supports the drift, could have been the bottom of the ocean without a plain record being left to prove it in the organic remains. I think it therefore quite illogical to assume a submarine condition of the surface, at any epoch, when we do not find immediately upon that surface some of the imperishable signs here mentioned of the action and presence of the sea.*

I deem it of the more importance to call attention at this time to

* These views were presented by me, in refutation of the Iceberg Hypothesis of the Drift, in the spring of 1844, in an Address to the American Association of Geologists and Naturalists.

the importance of ascertaining from independent and conclusive evidence, whether the strewing of the drift was in the strict sense submarine, because able writers, and among them some eminent geologists, have recently produced very interesting views of the drift and terraces of Northern Europe, all founded upon this assumption.

Let us, therefore, examine the actual proofs of the alleged submarine condition of the surface on which the drift of America and Europe was strewn, and see how far they conform to the distinctive tests of a genuine and long submersion.

Between the *main or lower drift* and the polished and furrowed surface of the solid strata, in the northern latitudes of North America, I am not informed of the existence of *any true marine deposits*, unless, indeed, we must assign to this position the very local beds of gravel with sea shells found by Desor, close to the shore of the Bay of New York, at a height above the tide of about thirty feet, and even these appertain most probably to the next succeeding interval, namely, that of the depositon of the clays, &c., of Lake Champlain. In this universal absence, then, of all the legitimate marks of the sea's previous presence at the time of the grooving of the rocks and first dispersion of the boulders, it is obvious that this whole district of the continent, now covered with the drift, stood at least as high as it does at present, and when we consider that throughout the regions bordering the southern margin of the drift, there are no traces of any of the late Tertiary formations, which next preceded the drift, and which would inevitably have been there deposited, had the continent at this period been depressed, it is possible that the land stood at even a greater elevation.

But reposing upon the earlier drift, and upon its scratched and polished floor, are the marine blue clays and yellowish sands of the middle and quiet period of the drift, occupying the less excavated parts of the great continuous valley of the St. Lawrence, Lake Champlain, and the Hudson River. These beds containing, as already said, many marine shells, and being unquestionably the sediments of an extensive ancient strait, do attest a submerged condition of the districts they overspread. But they are of extremely restricted limits compared with the whole area covered with erratic matter. They are confined to the great valley or plain just mentioned, and to the valleys of the Upper St. Lawrence, of the Mohawk and of some of the rivers of New England, but they do not extend to even the borders of Lake Ontario, and nowhere spread over the upland districts or general surface of the portion of the continent where they occur. Their

greatest elevation above the sea does not surpass three hundred and eighty or four hundred feet. From Lake Champlain, where they have this altitude, they decline very gradually in elevation towards the present sea coast, as we go either southwardly down the valley of the Hudson, or north-eastwardly down that of the St. Lawrence. At Albany the surface of this stratum is about two hundred feet above the tide, and below the Highlands it seems not to amount to one hundred feet. In the other direction the mean height of this continuous formation, identified by its fossils and composition, does not exceed, as it extends along the St. Lawrence from the Ottowa to Quebec, one-hundred and fifty feet, and I feel quite assured in saying that it nowhere amounts to two hundred. Having examined with considerable care since Sir Charles Lyell made his visit to that region, the relations of this fossilliferous stratum to the real drift with which it is in contact, I have satisfied myself that this eminent geologist was in error in classing with this strictly submarine formation, the small mass of gravel and fragmentary shells seen by him at an elevation of five hundred and forty feet above the sea, on the Montreal mountain. The materials in this gravel bed are such as to prove beyond a doubt that they could not have been deposited where they are, under a quiet sea, like that which filled at a level lower by four hundred feet the St. Lawrence valley, but that they were swept with other similar neighboring accumulations up the flank of the hill and lodged in a depression, during the transport of the second or later drift. Interspersed through gravel, not characteristic of the clay deposit of the plain below, are numerous *pebbles of that clay*, some of them imbedding the valves more or less entire of its peculiar shells. Indeed it is very difficult to find the fossils in this partially preserved condition, except where they have been thus protected: all the naked fragments met with among the sand being only the stronger hinge portions, and many of them being but little larger than its particles. The evidence, in fine, is positive, whether derived from the nature and conditions of the materials, or from the style of their stratification, and the very shapes of these shells containing patches of gravel, that they were thrown there by a surging current driving them up the mountain. If this correction of preceding observations be admitted, then is there not one proof remaining of the permanent submersion of the plain of the St. Lawrence under the sea, beyond a depression, compared with present levels, of about two hundred feet? It would thus appear that the maximum elevation was in the region of Lake Champlain, and

that even there, where the level was four hundred feet lower than at present, the proportion of land flooded by the sea, compared with the part exposed, was extremely insignificant. Had the depression in the region of Lake Ontario equalled that of the basin of Lake Champlain, it is obvious that the waters of the Atlantic must have filled the great Ontario valley and reached even to the foot of the Falls of Niagara. But with the facts now before us of the much lower level of the St. Lawrence submarine deposits, we cannot suppose this state of things, unless indeed we also assume an uniformity in the amount of the subsidence which the inequality in the amount of the subsequent upheaval to present levels, does not at all warrant, and which furthermore is incompatible with the almost universal tendency of the earth's crust to take on flexures, when disturbed in level by paroxysmal movements, and such must have attended the desiccation of the post-pleiocene clays and the dispersion of the later and more local drift.

It is, therefore, in strict accordance with the inference here deduced, from the low level of the fossiliferous clays and sands along the St. Lawrence valley, — the inference that the sea did not at that time penetrate to the basin of Lake Ontario, — that nowhere around the borders of that Lake, and indeed nowhere in the general region of the great Lakes, have any marine remains, or any genuine proofs of a quiet sea of this epoch, yet been met with. The boulder clay of both sides of Lake Ontario, so extensively spread out, and so easily examined in New York and Western Canada, has hitherto failed to disclose a solitary fossil, distinctive of the beds of the St. Lawrence and Lake Champlain. It is indeed a wholly different material, full of gravel and boulders, imperfectly stratified, and denoting in all its features an origin in turbulent waters; it is, in truth, but the inferior portion of the universal lower drift.

Along the more eastern of the river valleys of New England a formation of apparently the same age as the Champlain clay, and of similar origin, occurs in many places, especially near the Atlantic Coast. On the Penobscot River, in the State of Maine, its greatest elevation above the tide, as stated by Dr. Charles Jackson, does not exceed seventy feet; while I have myself observed it in the vicinity of Passamaquoddy Bay, containing some of the same marine shells, — *Saxicava rugosa*, *Tellina Greenlandica*, &c., — at an elevation of even more than twenty-five or thirty feet.

Reasoning from all that I have stated, of the very local distribution of this genuine submarine deposit of the era of the drift, and from

the very limited amount of vertical depression of the continent, which it implies, the conclusion is, I think, unavoidable, that almost the whole of the vast erratic formation of the United States was transported to where it now reposes, not upon a *submarine*, but upon a *continental* surface.

I might go on and examine in detail the facts connected with this assumed submersion of the great drift-covered plains of Northern Europe, and show that in all these localities, where terraces exist, the proofs of submersion are fallacious in like manner. If I were to read to you the original description of the Snowdon locality, it would appear that the evidence proves that a powerful current swept the materials of the drift across the Irish Channel and up the mountain. I hesitate not to say, that going over Northern Europe, you will find no legitimate evidence of a height of ocean greater than about one hundred and fifty feet above the Baltic. You will find that geologists have by no means distinguished between a quiet sea-bottom, and the materials formed by a wide rush of waters across the land.

It has not been my purpose to advance any creed of my own, but simply to present some of the test phenomena by which we can discriminate between the action of ice, and of water, and thus learn from the facts the real truth in relation to this assumed submersion, and in relation to the soundness of the Glacial Theory.

Prof. Agassiz, and other glacialists, have from time to time presented us with certain evidences, as they have deemed them, of the sole action of ice, independent of water. Inasmuch as the ice is essentially a plastic mass, it moves faster in the middle, precisely as the current of a river flows; and every fact dependent wholly upon this mode of progression will be valueless as a distinctive mark between the two. But there are tests which I think are decisive; and the beautiful map of Prof. Guyot, before you, affords me an opportunity of illustrating those tests.

[Prof. R. then explained, by reference to the map, the distinctive action of a glacial, and of an aqueous current, contending that both agencies were discernible in the Alps and the Jura.]

He proceeded to explain how an uplifting of the polar icy seas would give the general southern direction to the Drift, visible in Northern Europe, while a local centre of action in Central Scandinavia, would modify the distribution. This all-powerful force would be sufficient to propel the granitic boulders eight hundred miles across the gently-upsloping plain of Germany, Poland, and Russia.

Even in the gorges of the Jura, the position of the most eroded surfaces shows that the waters came, not from the side of the Alps, but from the North. Though the sea-level is now lower in Northern Russia by one hundred and fifty or two hundred feet than it was then, those arctic ices and floods could even now find their way across Northern Europe to the Ural Mountains and to the Alps, under the mighty impulse of earthquake undulations.

I am not one of those who wish doctrines to be enforced by an appeal to great names. Science is best prosecuted by a simple and modest adherence to the great teachings of nature, in preference to a following of nature's greatest expounders. Let us first go up to the great revelation itself, and then, if it be necessary, we may resort to its fallable commentators; the philosophers of the world. I do not therefore aim at supporting the points, upon which I have insisted, by the weight of great names; I have endeavored to confine myself to the determination of decisive and distinctive tests, but the subject admits of so much latitude that it has been with difficulty that I could refrain from exceeding my proper limits.

A short discussion ensued, during which Professor AGASSIZ, said that however correct the theory of translation by waves might be for other places, it would not apply to Switzerland. He had satisfied himself by what he had seen in the gorges of the Alps, and the plains between the Alps and Jura, that there was no possibility of an inducement for him ever to change his views upon this one point, for the arrangement of materials there was such, that it was physically impossible that water action could have had any thing to do with it.

ON SOME FOSSIL REMAINS FROM BROOME COUNTY, N. Y. BY W. C. REDFIELD.

Mr. W. C. REDFIELD read a paper on some mammalian remains of the genus *Vulpes*, which were found in fine clay, beneath drift, in the elevated ground which separates the upper courses of the rivers Delaware and Susquehanna, near the line which divides New York from Pennsylvania.

In the autumn of 1848 he had received from Major Brown, Engineer in Chief of the New York and Erie Railroad, the lower jaw and other bones of this mammal, partly enclosed in a matrix of fine clay, in which material the whole had evidently been embedded. These bones had been obtained by Mr. Jonathan Case, in excavating for the

line of railway at the Gulf Summit, in Broome County, N. Y., at a depth of forty feet below the natural surface, at an elevation of thirteen hundred and seventy-five feet above tide. The incumbent materials consisted, mainly, of gravelly clay and fragments of the native rock, which belongs to the Hamilton group of the New York geologists, and are such as constitute the greater portion of the drift in that region. A fragment of the New York corniferous limestone containing *Atrypa reticularis*, the outcrop of which rock is seventy miles distant, was taken from the incumbent drift near the spot where the bones were found.

Having made several visits to the locality, he had examined the ground and the evidence of the exhumation with great care, and found nothing to warrant the idea that the bones could have been buried by a slide of the surface materials, from the higher positions in the vicinity. By the favor of Maj. Brown, and through his assistants, Messrs. J. S. Beggs and J. Hilton, he had been furnished with profiles and cross-sections of the deep-cutting in which the bones were found, with accurate measurements, which, together with the bones and portions of their matrix, he now submitted to the examination of the section.

Mr. Redfield also presented specimens of fossils taken from two boulders of rock, in the drift, near Newark, N. J., which belong, severally, to the Delthyris limestone and the Oriskany sand-stone, of the New York system. These boulders must have had their origin at a point not less distant than the valley of the Rondout, the nearest outcrop of these rocks; having thus been carried over the mountain elevations of the Shawangunk and the Highlands by the active agencies of the drift period.

The outcrop from which these drift fossils were derived, bears from N. to N. N. W., bears from the place where they were found; the distance being, perhaps, seventy miles. The outcrop which furnished the *Atrypa* above mentioned, bears north-north-westerly from the summit where the bones of *Vulpes* were found.

On the Trails and Tracks in the Sandstones of the Clinton Group of New York: their probable Origin, etc.; and a Comparison of some of them with *Nereites* and *Myrianites*. By Prof. James Hall.

These trails and tracks occur on thin layers of sandstone, alternat-

ing with skaly beds in the lower part of the Clinton Group, in the central part of New York. The most conspicuous, are those presenting curved or meandering lines or furrows upon the surface of the sandy bed. From their character, and the character of the surfaces presenting them, they appear to have been made when the bed was exposed above water, or beneath shallow water only.

It has already been shown, in the Medina sandstone period, that the wave lines and stranded shells upon the surface of the thin sandy layers, furnish evidence of beaches and shallow water. The position of these trails is a little higher in the series, and in the succeeding group; the conditions being nearly the same as those during the deposition of the Medina sandstone.

The general character of the trails here noticed, is that of a meandering furrow, more deeply depressed at the two sides, elevated at the centre, and margined by a slightly elevated ridge of sand, which appears to have been pushed outward in the progress of the animal. Others of them are a simple furrow with the deepest depression in the centre; while others are fimbriated or ciliated along their whole extent; proving that they were produced by several distinct species of animals. In their general character, some of them are not unlike the trails made by *Natica* or *Littorina*, and others are more like the meandering trails of Idotea. In many, the great length of the meandering line, which crosses and re-crosses itself many times, seems to indicate that the animal moved rapidly over the surface. In others the trails are larger, and the animal probably moved much more slowly, the length being often limited to a few inches. These trails vary greatly in size, from a diameter of half a line to half an inch, and the smaller ones as a general rule, are the longest, and show more re-crossings than the larger ones.

Prof. HALL said he had been inclined to refer many of these trails to gasteropedous mollusks, while others are, perhaps, due to crustaceans, analogous to Idotea. These trails may have been made upon an exposed beach, as similar trails are on modern beaches, or they could have been made beneath water, since *melania* and other mollusks make similar meandering trails or furrows beneath shallow water. The fimbriated trails, however, are to be regarded as being made by a different animal, the character of which is not very apparent.

The specimens which I have here, showing the characters of these

trails, both of the smaller and larger varieties, leave no doubt as to their origin.

There is also here a very fine slab of sandstone, with a fimbriated trail upon the surface. That this marking was made by some body in motion, I regard as certain, not only from its association with the other plain tracks, but also from its general character. In this specimen also, the animal has re-crossed its own trail several times, obliterating the previous one at that point, and pushing forward the elevated margins, which could only have been done by some animal dragging itself over the surface. Several other specimens of similar trails are also shown upon other specimens, some of them more distinct than the first one, though of less extent, and one much resembling the impression of a narrow-leaved fern.

These trails are not only exceedingly interesting in their geological associations, and as showing the preservation of apparently slight and trivial markings, but they present interesting subjects of inquiry in reference to the comparison with other remains of very similar character, which have been referred to the Annelida. I mean the *Nereites* and *Myrianites*, first described by Murchison in his Silurean system. I have had no opportunity of seeing the actual specimens there described, nor any from those localities, but I have examined all those described by Prof. Emmons from the rocks of Maine, (specimens of which were shown to the Section.) Many of these are very similar to the fimbriated trails just described, and there are, moreover, associated in the same slabs, other markings similar to the plain and simple trails referred to Gasteropoda, and which possess no fimbriæ or lateral appendages. Among these Nereites and Myrianites, it will be seen, also, that many of the larger and more conspicuous forms are short, while the smaller ones are much more extended, and meandering over a large surface. These so regarded fossil bodies preserve no remains of an organic body, being on one side a simple longitudinal depressed furrow margined by fimbriæ, while the opposite layer presents a simple cast of this furrow and appendages. Many of them, barely discernible on the surfaces of the laminæ of the slate, present no appreciable difference of color or character from the surrounding material, and preserve far less evidence of the presence of an organic body than the Graptolites of the altered slates of Hoosic, in New York.

I will also mention, in this connection, that the genus *Nemapodia*, described by Prof. Emmons as a congener of *Nereites*, and found in

the silicious slates of Washington county, New York, has been proved to be the trail of some existing animal, made in passing over the exposed surfaces of the laminæ, and producing a slight discoloration and the destruction or removal of the minute Lichen which covered the rock. This fact is mentioned, merely to show how slight are the characters preserved, since this modern trail, probably of a *slug*, had been regarded as analogous to the others, and being the remains of a fossil worm.

In continuation of this subject, I would say, that there are other markings in connection with the trails I have described. These markings are not continuous, but appear as if made by some animals possessing hooks or claws at the extremity of the organs of motion. The impressions, or tracks in question, often appear as if made by a hook or claw, planted in the sand and drawn obliquely backward or inward on either side, as if to assist the animal in its forward motion. Other impressions present the marks of three, four, or five hooks or claws, arranged like the impressions of toes or fingers in the tracks of quadrupedal animals.

These impressions of toes or claws are so distinctly impressed and so well preserved, that there can be no question of the fact, but it is difficult to reconcile such markings and our general opinions of their origin with the fauna of the period. In reviewing the fauna of the period, we shall learn, according to our present knowledge, from the remains preserved, that there are no animals higher in the scale than fishes. It is shown to be impossible that crustaceans could make such markings, and *Trilobites* (the only crustaceans known to have existed at this period) do not possess appendages capable of making such impressions. No Saurian or Lizard-like animal — even admitting for the argument that they may have existed at that period — could have made footsteps with impressions of five toes. It seems, therefore, impossible to account for these markings by referring them to any of the known organic forms of that period. After endeavoring for a long time to account for them in a satisfactory manner, I have been induced to regard them as having been made by fishes as they propelled themselves forward over the sandy bottom in shallow water; that the impressions were produced by the rays of the fins, striking against sand as they were drawn backward. It will be seen that, instead of the impression of a palm or heel behind the five toe-like impression, there is a little elevated knot of sand, such as would be produced by an animal

swimming in water and striking the bottom, in this manner piling up a little ridge behind. This view is the only one which offers any satisfactory explanation of the facts, and, though little acquainted with the markings produced by fishes, upon the bottom of the sea or otherwise, he hopes, after stating this view, to obtain evidence from other sources to corroborate or invalidate the theory.

Prof. HALL also stated that he had sent several impressions of the engraved plates to Sir Charles Lyell, who had submitted them to Prof. E. Forbes, and that he had appended the latter gentleman's opinions in a note in his second volume of Palæontology. In reference to the trails before described, Prof. Forbes thought some of them like those of Gasteropeda, others like Planarian worms, while some of the clam-like impressions were similar to the tracks of fishes.

This elicited an animated discussion, particularly between Prof. AGASSIZ and Prof. ROGERS, upon the subject.

Prof. ROGERS inquired whether Prof. Hall regarded these wave lines as having been made on a beach or shore, and suggested that they might be made at great depths as well.

Prof. HALL explained that these lines differed from all the varieties of ripple marks, and were such as could not be made beneath water, referring at the same time to the similar markings or lines of sand bordering and making the outline of the wave upon recent beaches.

Prof. AGASSIZ remarked, that he had examined the locality in Lockport, mentioned by Prof. Hall, and had become satisfied that they were the lines of broken shells and other materials, pushed forward by the advancing wave, and illustrated their position relative to the ripple marks, trails, stranded shells, pebbles, &c., which had before been alluded to.

Prof. ROGERS objected to the view advanced, since he was able to trace the same kind of marking on a great part of the same formation in Pennsylvania, and admitting this origin, we should have a very extensive beach.

Adjourned.

J. B. FELTON, *Secretary.*

Fifth Day, August 18, 1849.

SECTION OF ZOÖLOGY.

This Section having met in University Hall, was organized by the appointment of Dr. H. I. Bowditch, as President for the day, and of Dr. W. I. Burnett, as Secretary.

On some new Points in the Morphology of Cells, touching its Analogy to that of the Ovum. By Dr. W. I. Burnett.

The time has long been passed when there could be raised a question as to the physiological value of cells as the basis of all organized structures. But if the discussion on this point is at an end, there is still a wide field left open to the investigator, particularly as to the tracing the laws of the formation of these cells, and their analogies in almost every point of view with the different elementary processes of reproduction.

It is a fact quite remarkable in science, that the discovery of these minute particles as being the basement work of all organisms, should have in many respects so completely exhausted the subject, that few are willing to prosecute it, and reap the meagre harvest that *seemingly* remains.

Dr. Schwann in 1839 published to the world his original inquiries on cell-life, and its relation to organized structures. Since then nearly the whole zoölogical world have fallen in with his views. In this comprehensive work certain laws of cell-development are laid down, based upon an exceeding number of trustworthy observations. These have become so familiar to every naturalist, that a recital of them might be unnecessary; but as my own observations in this direction, will not be, in that case, perhaps, so well understood, I shall briefly recapitulate Schwann's conclusions.

1. Cyto-blastema, or organizable liquid, exists.

2. Dark points, or minute solid bodies, are seen in this cyto-blastema — these are nuclei.

3. From these nuclei there begin to arise delicate membranes, which stand off from the nuclei as a watch-glass from a watch. This membrane gradually extends around the nucleus, and encloses it in a shut sac, the nucleus being still attached to its wall.

According to Schwann and Schleiden, this comprises all of the morphology of cells; and in their later works, and especially those of Schleiden, they distinctly affirm that they have since seen nothing which would lead them to take a different view of the formation of these minute bodies. According to these worthy observers, a nucleus always exists first, or if that nucleus be a hollow sphere, a nucleolus even before the former; and from and around a nucleus, therefore, the cell-membrane is always subsequently developed. They therefore make quite a distinction between the real nature of the nucleus and that of the cell, and do not regard the latter as simply the adult development of the former.

It is quite pleasant, I am well aware, to have arrived at an ultimate unity of a grand series of subsequent phenomena — and, were the theory of Schwann and Schleiden of universal application, we might well rest satisfied, as having reached the ultimate processes of the organized world. But for some time I have been in doubt as to the universal application of these laws of cell-formation; and some observations which of late I had the good fortune to make, have fully settled in my mind that animal cells arise in a manner wholly different from that designated by Prof. Schwann. These observations were made upon some cellular tissues of rapid growth, in which I had an opportunity to watch the various phases of development, from the lowest to the highest.

In the first place I will state, that through a long course of observation, I have never been able to perceive cells arising *from* a preëxisting nucleus, according to the theory of Schwann. On the other hand, they have always appeared to arise according to this, which I call a new method, and which I will now describe.

These observations were made upon the epithelial cells of mucous membranes, and upon pus-cells, the result of inflammation.

In the first place there appears in the cyto-blastema a dark point or nucleus.

2d. This nucleus is next seen to be a hollow sphere, containing nothing but a clear liquid.

3d. This clear liquid is cloudy and filled with minute, immeasurable granules.

4th. The cloudiness is less marked, and in some cases nearly absent — but a dark body is seen in its centre.

5th. By this time the cell-membrane, or the walls of the primal

nucleus hollowed out, has much enlarged; so that we have then a distinct nucleated cell.

6th. This nucleus then begins to be hollowed into a sphere, as did its predecessor, and then passes like that, from a clear to a cloudy vesicle, until the formation of its own nucleus. By this time the outer cell-membrane bursts, having completed its term of existence: this having taken place, the nucleus is discharged, and we have then a *nucleated* cell, as before. Sometimes, however, the outer cell-membrane does not burst; we have then a *nucleolated* cell; but this is not the most common manner of proceeding.

Such is the biography of a cell, according to this mode of formation; and it differs from that of Prof. Schwann, especially in admitting that, as to real nature, the *nucleus*, in these cases, differs not from the *cell*, except in its younger and more imperfect condition, and that the nucleus is formed *after* the membrane. How far this mode of cell-formation is applicable in a general way, future and more extended inquiries will determine.

It now remains for me to refer for a moment, to an anomalous mode of the reproduction of these cells. The increase in the number of cells takes place usually, I think, by the endogenous method just mentioned — that is, that a cell is formed within a cell, &c. — by a mere repetition of the first process. But with some cells, and particularly those belonging to epethelial structures, I have observed another mode in their propagation. The following is the process: —

After a cell with its nucleus is formed, and the nucleus is filled with granular contents, there begins to appear on one side of this nucleus, now properly a cell, a slight sulcus; this deepening, the cell is finally divided in two, and the new cells thus formed have all the anatomical characteristics of the old. In one instance I think I have seen one of these subdivided cells divide again; but a single scission is most often observed. It should be remembered, in this connection, that these were purely animal cells, and having no claim to a higher grade of organization than any other simple cells of the animal economy. These two points necessarily call up in the mind some pleasing analogies of the processes here passed through, with those occurring in the formation of the ovum. I refer here not to those of the ovum described by Wagner, but to some observed for the first time, I think, by Prof. Agassiz. It was early in the Spring of 1848, that this last named gentleman made quite an extensive series of observations upon the development of the ova of Radiata and Mollusks. The results of

these observations, as far as relate to the matters now at hand, are briefly the following:

1st. Dark points or nuclei in a homogeneous matter secreted from the ovary and contained in vesicles.

2d. These nuclei become hollow vesicles, filled with a clear liquid.

3d. This liquid has become densely cloudy.

4th. Granules begin to appear in this cloud.

5th. In the midst of these granules, and apparently by a kind of condensation, a nucleus appears.

6th. This nucleus has a dark point or spot.

We have, then, the egg, with all its usual constituents. The *outer vesicle*, with its *granular* contents, being the *vitellus* — the *nucleus*, the *germinative vesicle;* and the *dark point* in it, the *germinative spot.*

It will immediately be perceived how identically the phases here passed through, as described by Prof. Agassiz, agree with those observed by myself as occurring in *animal cells*. In fact the grand law of development here sketched out, seems to be the same in both cases.

Then, again, if it is borne in mind that, in these observations of my own, a scission or fissuration of cells was seen, exactly corresponding to the division of the vitellus, after the fecundation of the ovum, we shall have the Morphology of an animal cell, and that of the ovum, brought upon the same level; although it will be admitted that physiologically they appear somewhat different; and certainly it would seem that we had gained a point of some importance in Histology and Embryology, by thus bringing these two points together; and especially as indicating that development, growth, and reproduction — in fact the whole philosophy of life — rest upon a single fundamental law in the production of cells.

The identity of a cell and an ovum has heretofore been strongly insisted upon, not so much from observation as on account of their apparent similarity of structure; but I am unaware that any one has heretofore traced from actual observation their morphological identity.

I cannot here dismiss this subject without at least alluding to a collateral matter, which certainly is pleasing to the imagination, if not important as a matter of science. When Prof. Agassiz had made in the spring of 1848, his most important discovery as to the primal formation of the ovum, a very pleasing *seeming* analogy arose in his mind. It was the analogy of the *nebular* hypothesis of the formation of worlds, with the formation of these simple elementary particles of

the organized world. And if I may be allowed to express in my own language the idea of another, the analogy in detail would be the following: —

The clear, hyaline liquid of the vesicle, would represent the so-called "all-pervading ether" of space. The subsequent cloudiness of this liquid of the vesicle would represent the appearance of minute particles of matter in an aeriform condition. And the condensation of this opacity into a solid nucleus, would typify the so supposed condensation of minute particles in space into a world.

But there is another fact which makes this analogy still more striking. It is, that after the ovum has thus been formed, it begins to revolve, from some unknown power, upon its own axis, and continues so for quite a length of time. So that, allowing the imagination to be carried away for a moment with the beauty and force of this *seeming* analogy, and throwing aside all controversial points as to the nebular hypothesis, we have portrayed in the minutest particles of the organized world, both in *ova* and in *animal cells*, the philosophy of the formation of the systems of worlds around us.

This last I do not bring up as a matter of pure science, but as one of the happiest of those analogies which constantly arise in the minds of naturalists, bearing upon the *apparent* unity of *idea* of every thing around us.

Prof. Gray expressed his gratification with the facts stated in Dr. Burnett's paper; and proceeded to make a few remarks upon the formation of cells in plants. He said that vegetable physiologists had not been satisfied with Schleiden's view of the formation of the cell, and he had noticed that Schleiden himself had of late appeared to be dissatisfied with it. As to the fissiparous mode of cell-production, spoken of by Dr. Burnett in the above paper, he remarked that with vegetable cells this was the most common mode of increase, and some had maintained that it was the only method. Prof. Gray then referred to the discoveries of Mohl in this direction, which had at length convinced Schleiden that cells do multiply by scission.

Prof. Agassiz remarked that he was satisfied that the reason why no essential progress has been made since the publication of Mr. Schwann's paper has arisen from the fact that cells were taken for the same cells wherever they were found, instead of studying the cellular tissue in the vegetable and animal kingdom separately. The cells of allied animals should be compared, to ascertain whether there

are not particular cells corresponding to particular types of animals, so that we may recognize different animals by their cells, as they may now be recognized by their bones. If this be so, a far more extensive field of inquiry is opened to us than was developed by comparative anatomy half a century ago.

The discoveries of Schwann showed that animal tissue consists of cells as well as vegetable tissue. But the botanist and the zoölogist must each study their subjects separately. The cells of the animal tissue differ strikingly at different periods of life, yet there is a resemblance between the cells of the youngest and of the full grown tissue. There is a complete identity of structure between the egg in its formation and the formation of cells in full grown tissues. The cells in full grown animal tissue are for the purpose of reproducing tissue. The food is formed into cells, and a great part of it passes off at the surface.

The cells produced in the full grown animal are of the same kind with those which existed in its earlier period of life. In the egg it is not so. We have first an isolated cell. Out of that cell must be built up a new individual, and it must be formed by the successive growth of new cells, which furnish the organs of which it consists. Thus, in the egg the cell produces between the nucleus and its outer wall a new generation of cells. These cells are the yolk of the egg. In the centre remains still the nucleus, which may produce further developments.

The cell which produces the egg may grow to a large size, but the cells of vegetable tissue are microscopic. The egg cell may be surrounded by other materials, and this again by a shell to protect the whole. We never see a structural cell surrounded by a hard envelop. In structural cells, as soon as there is a third generation of nuclei, it bursts, but the germinative cell of the egg does not die, nor the vitelline membrane, even after a fourth generation of cells. The granules in the cloudy matter of the yolk are transformed into larger granules, each of which gives rise to cells; so that myriads of cells may be formed in a short time.

Another difference between the ovarian and the structural cells is, that the contents of the ovarian cells may be dissolved again into a homogeneous liquid, at a certain period of development. This dissolution takes place in a certain stage of the egg's growth. In almost all eggs, after a great amount of cells in the vitelline membrane has been formed, the germinative vesicle is dissolved and its contents

flows into the vitellus, which undergoes a new state of transformation. This is the period of the first appearance of the germ.

The changes which celestial bodies undergo, as far as astromomers have traced them, agree with the changes which we observe in the growth of ovarian rather than of structural cells. This is because these condensations in space are the process by which worlds are called into existence, and not that by which worlds are sustained. The differences which Prof. Agassiz thus pointed out between the ovarian and the structural cells he thought confirmed the view with which he started.

ON SOME BONES ON THE DINORNIS NOVÆ ZEALANDIÆ.

BY PROF. CHASE.

Prof. CHASE, of Brown University, exhibited some of the bones of the *Dinornis Novæ Zealandiæ*, upon which he made the following remarks: —

These bones are from New Zealand. They were placed in my hands by G. M. Fessenden, Esq., of Warren, R. I. He received them from Capt. Mayhew, of the whaling bark Powhattan, who passed several years on the north island of this group, and to whom on leaving it these bones were presented by the Chief of the island. They were portions, as the Chief informed him, of the skeleton of a huge bird, which though now extinct, was remembered by the oldest inhabitants. They compared it in size to a horse. It was exceedingly ferocious, and so powerful as to make it dangerous for a single person to encounter it. Indeed, so many lost their lives in such encounters, that the inhabitants of the island at length made a general war upon it, and after much hard fighting effected its extermination. Such is the account given by the Chief, of this gigantic bird — an account entitled, it is probable, to about as much credit as the analogous traditions which prevailed among the aborigines of our own country in regard to the extinct mammoth.

Captain Mayhew states that he had for several months on beard his vessel a box containing a large collection of bones belonging to this bird, including nearly every part of the skeleton. This collection had been made by the Rev. Dr. Williams, Missionary at Poverty Bay, and was afterwards sent out by him to England. It was undoubtedly one of the boxes which Prof. Buckland received from Dr. Williams, and

from the contents of which Prof. Owen prepared the papers read by him in 1843 and 1846, before the London Zoölogical Society, on a new genus of struthious birds, for which he proposed the name of *Diornis Novæ Zealandiæ.* The bones were found buried in the mud deposited by the fresh water streams which come down from the mountains. Many of them were in a very perfect condition, so that Prof. Owen was able to determine the character of the bird from them with great precision. Since then still larger collections of the bones have come into his possession, not only confirming his previous inferences, but enabling him, in several instances, to extend them. The bones have undergone but little change in their composition, nearly all the animal matter being still retained by them.

As the bones which lie before me belong to more than one of the species of the *Diornis*, and are the first it is believed that have come to this country, a brief recapitulation of the principal characteristics of the genus, may not be inappropriate in connection with them.

In the first place the larger species of this genus were remarkable for their gigantic size. The *Diornis ingens* to which the principal bone upon the table belonged, must have been at least twice as large as the Ostrich; and yet this was only the second species in point of size.

The genus is also further characterized by certain peculiarities in the form, structure, and proportion of the several parts of the skeleton. The femur presents nearly the same proportions of thickness to length as that of the Ostrich. There is, moreover, no air hole at the back part of the neck, as in the Ostrich and Emeu, showing that, as in the case of the Apteryx, the interior of the bone was filled with marrow instead of air. It is not quite half the length of the tibia, which is a greater disproportion than is found in any existing birds of the order. The tibia is remarkable not only for its great proportional length, but also for presenting at the anterior concavity, just above the distal condyles, a complete osseous canal for the passage of the tendon of the great extensor muscle. This canal is not found in any of the other struthious birds. The crest of the tibia is also comparatively little developed. The tarso-metatarsal bone is scarcely longer than the femur, and consequently only about half the length of the tibia. In the Emeu, the tarso-metatarsal bone is as long as the tibia, and in the Ostrich nearly as long. In the Apteryx, the proportion between the two bones is the same as in the Dinornis. The distal

extremity of the tarso-metatarsal bone shows that instead of having two toes like the Ostrich, it was tridactyle, in this respect resembling the Emeu, the Rhea, and the Cassowary. In the Apteryx, there is a small fourth toe on the inner and back part of the foot, articulated to a slightly elevated rough surface of the tarso-metatarsal bone. The Dinornis, therefore, differs generically from the Apteryx, which it resembles more nearly in the structure and proportions of the leg bones, than any other of the existing Struthionidæ.

No wing bones of this gigantic bird have as yet been obtained. It is inferred from the keelless, shield-shaped sternum, that these organs existed only in a rudimentary state. The same inference may also be drawn from its solid skeleton, the air penetrating no part of it except the vertebral column. In the Apteryx, whose wing bones are so slightly developed that they can be discovered only by careful dissection, the air is wholly excluded from the skeleton. Prof. Owen therefore supposes the wings of the Dinornis to have presented a degree of development intermediate between that of the Ostrich and the Apteryx.

Prof. Owen refers the bones which have come under his examination, to six different species. In distinguishing these species he is guided principally by the different dimensions of the bones, which are known from the perfect coalescence of their primitively distinct elements to have belonged to adult birds. In all the Struthionidæ, the skeleton has a tardy ossification. The Ostrich, when two thirds grown, has the tarsal bones yet distinct from one another, and from the metatarsal, although they are all subsequently consolidated into a single bone. The species are also further distinguished by certain differences in the relative size and proportions of the several parts of the skeleton.

The largest species for which Prof. Owen has proposed the name of *Dinornis giganteus*, had a femur sixteen inches in length and seven and a quarter in circumference. The tibia was thirty-five inches long and six and a half in circumference, and the tarso-metatarsal bone eighteen and a half inches long and five and one half in circumference; making the entire leg when straightened, five feet nine and a half inches in length. The corresponding bones in the leg of the Ostrich measure three feet nine and a half inches, having severally about the same proportional thickness. The average height of the Ostrich is seven feet. If we suppose the other parts of the skeleton of the *Dinornis giganteus* to have borne the same proportion to those

of the Ostrich as the bones of the leg, its height must have been between ten and eleven feet; or if we suppose the proportions to have been those of the Cassowary, its height must have been between nine and ten feet. In either case its weight must have been three times that of the largest living bird. From the size of the distal extremity of the tarso-metatarsal bone as well as from that of two or three phalangial bones that were in the collection, Prof. Owen supposes the foot prints of the *D. giganteus* must have exceeded the largest of those which are found in the sandstone of the valley of the Connecticut. Fragments of the shells of eggs, belonging it is probable to the species, were also received along with the bones, indicating by their slight concavity the great size of the eggs. They are not pitted like the eggs of the Ostrich, but marked by short interrupted grooves, running in different directions. The young of the *giganteus*, when first hatched, must have been nearly as large as an ordinary turkey.

The second species — the *Dinornis ingens* — was intermediate in size between the *D. giganteus* and the Ostrich. This tibia, which belonged to it, is twenty-nine inches in length and six and a half inches in circumference at its middle part. The three bones forming the leg together measure four feet and seven inches. They were proportionally thicker than either those of the *D. giganteus* or those of the Ostrich. The height of the *D. ingens* may have been from eight to nine feet.

The third species in respect to size, is the *Dinornis struthoides*. It was about the height of the Ostrich, but of more robust proportions.

The fourth species is the *Dinornis dromioides*, supposed to be about as large as the Emeu, whose average height in captivity is between five and six feet.

The fifth species, the *Dinornis didiformis*, was about four feet in height, being somewhat larger than the extinct Dodo, which it resembled in the robustness of its proportions. This fragment of bone is a part of the tibia of the *didiformis*.

The sixth species, the *Dinornis otidiformis*, is believed to have been about the size of the great Bustard (*Otis tarda*) from which it is named.

Of the distribution of these several species among the different islands of New Zealand I am unable to speak, nor am I prepared to offer any remarks upon the circumstances which may have given rise to the development of so extraordinary a family of birds upon these islands. From the remarkable preservation of their remains, as well

as from the circumstances under which they are found, their extinction is believed to have taken place at a comparatively recent period. Like that of the Dodo, it was probably effected through the agency of man.

Upon the conclusion of Prof. CHASE's remarks, Mr. WELLS observed that bones of a similar character had been found, in red sandstone, in South Hadley, Mass., by some Irish laborers, while excavating a canal. Unfortunately they were destroyed; but the engineer, whose testimony was reliable in the matter, had informed him that some of the bones were larger than the leg bones of the horse.

Prof. AGASSIZ here spoke of the importance of preserving even the smallest portion of bones of this character; as, by the aid of comparative anatomy, the mysterious track in the red sandstone might thus be explained.

Dr. GIBBES exhibited some specimens of the only fossil birds' bones whieh have been discovered in this country. They came from South Carolina.

Prof. AGASSIZ, alluding to the tradition mentioned by Prof. CHASE, with regard to the bones he had exhibited, said that he thought such traditions afforded no evidence that these extinct species of animals had existed within the historical period. Similar traditions existed, in Siberia, with regard to the Mammalia found there; and like traditions also were common among the North American Indians. But there is no geological evidence of the existence of man with extinct species of animals.

To this Mr. MANTELL replied, that such evidence had recently been discovered. Bones of this character had been found, by his brother, in the bed of a stream, in some loose sand, where evidently was once the channel of a river. Digging down, he found the evidence of extinct fires; and in these charred places were found bones of this character, together with human bones, those of a dog, the remains of shell-fish, and fragments of egg-shells curved in the contrary direction by the action of fire. The skin and beak of this monster bird had been found in this place. The reason for supposing the animal to have been contemporaneous with man was, that the bones presented a white appearance, which can only be produced by burning the bones while they contain animal matter. Mr. Mantell's father (Dr. Mantell, of London,) has recently read a paper on this subject, before the Royal Society, at London.

On the Metamorphosis of Fluvicola Herricki. By Dr. Le Conte.

Dr. Le Conte made some remarks on the transformation of *Fluvicola Herricki, De Kay*, into the pupa state. A similar animal was known many years before to European zoölogists under the name of *Scutellaria amerilandica*. The present species was observed in great numbers at Niagara, at the middle of June. Before changing into the pupa state, they leave the water and attach themselves to the under surface of stones at the edge of the rapids. The elliptical shield, by means of its ciliated margin, adheres closely to the rock, being cast with the larva skin, and forms a protection under which the subsequent changes take place.

The homologies between Fluvicola and the ordinary water-breathing larvæ were then pointed out; the only difference being the prolongation of the dorsal epidermis beyond the margin of the body. Similar prolongations were instanced in several orders of insects. Dr. Le Conte was still in doubt respecting the nature of the perfect insect produced from this pupa, but was inclined to believe it belonged to the order Neuroptera. Further investigations have shown it to be truly Coleopterous, and belonging indeed to the genus *Eurypalpus Dej*, a curious genus of aquatic habits, but allied to Cyphon and Lampyris.

The meeting then adjourned to meet the Geological Section.

W. I. BURNETT, *Secretary*.

Sixth Day, August 20, 1849.

GENERAL SESSION.

The Association met at 10 o'clock, A. M. The minutes of the last meeting were read.

The following resolutions, recommended by the Standing Committee, were then passed: —

Resolved, That a special meeting of the Association be held at Charleston, S. C., on the second Tuesday of March next.

Resolved, That no paper be read before the future meetings of this

Association unless an abstract of it has been previously presented to the Secretary.

Resolved, That hereafter all books, charts, maps, and specimens, which may be presented to the Association, shall be given to the Smithsonian Institution.

The President announced the following Committee on Lieut. MAURY's communication on Winds and Currents :— President SPARKS, Prof. LEWIS R. GIBBES, of South Carolina ; Prof. BENJAMIN PEIRCE, Cambridge ; W. C. REDFIELD, Esq., of New York ; J. INGERSOLL BOWDITCH, Esq., Boston ; Prof. A. GUYOT, Cambridge.

A resolution for the appointment of a committee to report on the progress of the science of Mineralogy, offered by Mr. Moss, of Philadelphia, was referred to the standing committee.

The following resolutions were offered by Dr. Gray : —

Resolved, That a *manual or manuals of scientific observation and research*, especially adapted to the use of the American inquirer, comprising directions for properly observing phenomena in every department of physical science, and for making collections in natural history, &c., whether on land or at sea, is much needed at the present time ; and that such a publication placed in the hands of officers of the army and navy, of voyagers and travellers, would greatly tend to develop the natural resources of our extended country, and to the general advancement of science.

Resolved, That the American Association for the Advancement of Science cordially recommends the Smithsonian Institution to undertake the preparation of such a volume, under the editorial superintendence of its Secretary, to be published in its series of reports.

Resolved, That this Association will cordially co-operate in the production of such a manual, or manuals, in whatever manner may be best adapted to secure the end in view.

These resolutions, after some discussion, were referred to the Standing Committee.

The Secretary then announced the programmes of the Sectional meetings for the day.

Adjourned to meet in Sections.

E. N. HORSFORD, *Secretary*.

Sixth Day, August 20, 1849.

SECTION OF MATHEMATICS, PHYSICS, AND ASTRONOMY.

Prof. SNELL, of Amherst was elected chairman.

A communication was made on METEOROLOGICAL INSTRUMENTS, by Prof. GUYOT.

[Not received.]

A COMMUNICATION ON GENERAL TABLES FOR THE REDUCTION OF THE APPARENT PLACES OF STARS. BY PROF. HUBBARD.

[Not received.]

This communication was followed by remarks from Lieut. Davis, and Mr. Walker.

On motion of Prof. PEIRCE, it was voted that the Standing Committee be requested to appropriate as much time as possible of to-morrow for the discussion upon meteorological instruments.

Prof. MITCHELL, of Cincinnati, gave an account of his IMPROVEMENTS IN THE APPLICATION OF THE MAGNETIC TELEGRAPH TO ASTRONOMICAL OBSERVATIONS.

[Not received.]

This gave rise to an animated discussion between Prof. MITCHELL, and Profs. HENRY, PEIRCE, BACHE, and Lieut. MAURY.

At a quarter past 2 o'clock, the Section adjourned.

Afternoon Session.

The Section met in the Lecture Room of the Chemical Laboratory, and was organized by the choice of President SPARKS, as Chairman.

Dr. HARE presented his views on the THEORY OF TORNADOES.

This communication was followed by a discussion in which Professors HENRY, HARE, and JOHNSON, and Messrs. REDFIELD and ABBOTT took part.

Adjourned.

B. A. GOULD, JR., *Secretary.*

SECTION OF CHEMISTRY AND METEOROLOGY.

HENRY WURTZ, Esq., in the Chair.

ON THE DETERMINATION OF THE WATER CONTAINED IN CRYSTALLIZED SULPHATE OF QUININE, (DISULPHATE OF QUININE,) AND A METHOD FOR TESTING THE PURITY OF THE COMMERCIAL ARTICLE. BY DR. C. LINCK, Assistant in the Cambridge Laboratory.

LAST winter, during an investigation of some adulterated sulphate of quinine, I became aware that the formula of this salt, as given in later books, on the authority of Baup, was erroneous, it making the amount of water of crystallization too large, and consequently that of the quinine and sulphuric acid too small.

There are no instances on record of deceptions successfully practised by mixing cheaper sulphates with this drug; such adulterations being too easily detected, either by the eye alone, or at least by simple experiments. For this reason if analysis shows a suspected article to contain exactly the amount of sulphuric acid which is contained in pure sulphate of quinine, there can be no simpler or stronger proof than this of its purity. If, on the other hand, the amount of sulphuric acid is found to be less, it is evident that the article is impure. The accurate determination of sulphuric acid being a very easy matter, this method of testing the purity of that important medicine appeared to me to be very convenient, provided the exact composition of it was perfectly understood.

For this reason I entered upon a re-investigation of the subject, as soon as I found that the quantity of sulphuric acid in the pure commercial salt did not agree with the amount calculated from the existing formula of this compound.

I found three different samples of sulphate of quinine agreeing perfectly well in their composition, and containing 10.04 per cent. of acid, and about 9 per cent. of water, instead of 8.94 per cent. of acid, and 16 per cent. of water, which Baup's formula, $(2\ddot{O}\ SO_3\ HO + {}_8HO,)$ would require.

Pure commercial sulphate of quinine was re-crystallized, one portion from water and another from alcohol. The crystals were collected, quickly dried, and pressed between blotting paper. The water was determined by drying the substance first in a water bath, and ultimately in an oil bath at 130° C. The residue was then taken from the watch glass by dint of dilute hydrochloric acid, carefully rinsed

into a small beaker glass and the sulphuric acid precipitated by chloride of barium.—0.449 grms. of salt crystallized from water gave: water =0.038 grms. = 8.28 per cent., and sulphate of baryta = 0.1345 grms. corresponding to 10.06 per cent. of sulphuric acid. 0.819 grms. of salt crystallized from alcohol gave: water = 0.073 grms. = 8.91 per cent., and sulphate of baryta = 0.228 grms., corresponding to 9.57 per cent. sulphuric acid. 0.517 mgrms. of the same gave: water 0.0415 grms. = 8.03 per cent.; sulphuric acid not determined.

An article with the label of Dennis & Rosengarten, Philadelphia, without being pressed between blotting paper, yielded,

	I.	II.	III.
Water, . .	10.46 per ct.	10.63 per ct.	10.50 per ct.
Sulphuric acid, .	9.51 "	9.72 "	9.56 "

In order to see how far the different commercial articles might agree with each other in their composition, I determined the sulphuric acid in the following four samples, after having freed them from adhering moisture by passing them between blotting paper.

(A) French quinine, purified by re-crystallization.

(B) Quinine sold with the label "London Alcaloid Company."

(C) Quinine from Dennis & Rosengarten, Philadelphia.

(D) An article adulterated with mannite, French label.

(*a*) I. 10.11 per ct. II. 10.05 per ct. Mean, 10.08 per cent. sulphuric acid.

(*b*) I. 10.08 per ct. II. 9.98 per ct. Mean, 10.03 per cent. sulphuric acid.

(*c*) I. 10.08 per ct. II. 9.99 per ct. Mean, 10.03 per cent. sulphuric acid.

(*d*) I. 7.49 per ct. II. 7.32 per ct. Mean, 7.41 per cent. sulphuric acid.

The mean of the per centage of the three first samples is 10.04 per cent.; they were pure articles, and agreed well in their composition. The quantity of sulphate of quinine in the fourth is expressed by the fraction $\frac{7.41}{10.04}$ which gives a per centage of 73.8 of salt and 26.2 per cent. of impurities.

Efflorescence of the Salt. Several lots of pure crystals were exposed to the open air, in a moderately warm room, for the space of from a week to ten days. They yielded,

	I.	II.	III.
Water, . .	4.07 per ct.	6.28 per ct.	5.73 per ct.
Sulphuric acid,	10.30 "	10.35 "	10.33 "

It will be seen that nearly one half of the water which can be expelled in the oil bath is lost by mere exposure to the air; but it seems that about five per cent. of other water cannot be expelled without the aid of heat, for the lot which was examined first had lost more water than the subsequent samples.

In order to see whether after drying in the oil bath any water of constitution was left behind or not, the carbon and hydrogen of the compound were determined by means of combustion with chromate of lead.

I. 0.413 grms. of salt gave: carbonic acid = 0.977 grms.; water = 0.252 grms. C. = 64.40 per cent. H = 6.77 per cent.

II. 0.420 grms gave: carbonic acid, 0.989 grms.; water = 0.273 grms. C. = 64.24. H. = 7.22 per cent.

III. 0.3823 grms. gave: carbonic acid = 0.903; water = 0.237. C. = 64.41. H. = 6.88 per cent.

These results agree very well with those of the current supposition that sulphate of quinine after being thoroughly dried in the oil bath retains but one equivalent of water. Its formula is ($2\bar{Q}$, HO. $SO_3 = 2\ (C_{20}H_{12}NO_2) + SO_3$.)

It contains carbon and hydrogen.

	Calculated.	Found.		
C.	64.32 per cent.	64.40 —	64.24 —	64.41
H.	6.69 "	6.77 —	7.22 —	6.88

As to the quantity of water of crystallization which can be expelled by the oil bath from the original salt, a small calculation shows that four equivalents of it would amount to 8.79 per cent., a quantity which agrees well enough with the experiments stated above, ($2\bar{Q}$ HO. SO_3 + 4Aq.) must, therefore, be accepted as the formula of freshly crystallized disulphate of quinine. The quantity of sulphuric acid corresponding to this formula, is 9.80 per cent. Two equivalents of water of crystallization correspond with 4.60 per cent. of water that can be expelled, and this is the salt which remains when the first salt has been exposed for some time to a moderately warm atmosphere. Thus two equivalents of water are lost. Baup makes this loss amount to four equivalents, and Soubeiran even to six, probably in consequence of their having experimented with a preparation to which a great deal of moisture was still adhering. ($2\bar{Q}$ $HOSO_3$ + 2HO) calls for 10.25 per cent. of sulphuric acid. Baup and Phillips also say that they had found that sulphate of quinine, after having been dried at 100° C. gives out two more equivalents of water in an oil bath at 120° C. I

have persuaded myself that the salt, after having been exposed to a water bath for several hours, does not lose any more water when brought into an oil bath heated to 130° C.

Baup further states that disulphate of quinine fuses in its own water of crystallization when heated to 100° C. With none of the samples which have come under my notice have I been able to see this assertion verified.

Dr. F. BACHE, of Philadelphia, remarked that the subject was one of very great importance, as tending to protect us against an impure article of sulphate of quinine. But he thought there might be a defect in the method proposed by Dr. Linck. If there were any other article that would saturate sulphuric acid as well as quinine, it would not be detected. He did not recollect but one article, and that was salycine, which was a much cheaper article than quinine.

Dr. LINCK said he was aware of the objection made, but he had not been able to find any article which could be used for adulteration of sulphate of quinine, which would not be detected in the manner he had described.

He stated that salycine, according to the best authorities did not form any neutral salt with sulphuric acid, and that he could not think of any organic sulphate, cheaper than that of quinia, and liable to be used as an adulteration. Sulphate of ammonia and metallic sulphates when present, were easily detected by qualitative tests, so that nobody would venture to use them.

ON THE ELECTRO-DYNAMIC FORCES. BY PROF. LOVERING.

THE mechanical forces at the foundation of electro-dynamics are, 1. That currents which run in the same direction attract one another; and 2. That currents which run in opposite directions repel one another. It is understood that currents, not parallel are running in the same direction, if both move towards or both move from the point or line which represents their shortest distance from each other.

These forces combined with Ampere's electro-dynamical view of magnetism, are sufficient to explain, not only the action of currents upon currents, and of currents upon magnets, but also the polar forces in ordinary magnetism. These forces constitute the prime mover in all electro-magnetic machines. Under this description are comprised all those machines which are moved by any combination of currents

of electricity, as well as those which employ the force of iron thrown into a magnetic state by the electrical current. In one sense they are all electro-magnetic. For the attractions and repulsions of currents are all which need be supposed, in the last analysis, to explain magnetism.

Still, a general division of these machines may be made with advantage into two great classes. I. In the first class, I place those in which the axis of motion is so related to the other parts that the latter, which are attracted or repelled, may come into the direction of the acting forces. When this direction has been reached, the motion of such machines will cease unless a break-piece or pole-changer is used. II. In the second class, I place those machines in which the axis of motion is such that the motion does not essentially change the relative position of the attracting or repelling parts of the currents which produce it. In this case the motion will continue as it has begun, so long as the currents retain their vigor. In this class of machines, where one current is at right angles to the other, the force appears as a tangential one, though it is well known that it can be analyzed into forces of direct attraction and repulsion. When the currents are disposed in this way, they must act *all the time* very obliquely upon each other; on this account such machines are much more delicate, and require for their exhibition much more careful manipulation than those first described.

Each of these principal classes admits of a convenient sub-division into several varieties. 1. There may be two battery currents to produce the motion. 2. There may be one battery current and a steel magnet. 3. There may be a battery current and an electro-magnet. These three varieties are common to both of the great classes above mentioned. There are three others peculiar to the first class. 4. There may be two electro-magnets. 5. There may be an electro-magnet and a steel magnet. 6. There may be an electro-magnet and a revolving armature.

The second class of machines does not admit of an advantageous application of Schweigger's multiplying principle to make up for the oblique action of the elementary forces. They are and must remain extremely feeble in their operation. We need not be surprised to learn that it is the first class of machines which has been principally studied by practical men. These alone exhibit an energy of action which can be expected ever to compete with other mechanical agents. On this account the latter class of machines has been left to

be investigated, if at all, by men of science, and for their theoretical importance. Many improvements have been made in the apparatus used to repeat the experiments on tangential forces, which employ the 2d and 3d varieties of machines into which I have divided the second class. For the exhibition of the action of currents upon one another, especially in those cases where the forces act tangentially, little has been added to the electro-dynamical table of Ampere. In Ampere's apparatus, which has the merit of displaying the fundamental forces of electro-dynamics in their most elementary form, powerful batteries are requisite. The following contrivances are recommended for experimenting upon the tangential action of currents. In the facility of manipulation, and the rapidity of motion produced, they will be found superior to Savary's apparatus, as attached to Ampere's table.

The revolution of a wire frame, around the pole of a steel magnet or an electro-magnet is a familiar experiment. Theory indicates that, if the same frame were suspended in a similar way on one of the extremities of a compactly wound helix, traversed by a powerful battery current, a similar motion would be produced. Experiment confirms this deduction from theory. But the experiment is in this case much more difficult than the corresponding one which suggested it, inasmuch as the electro-dynamical force of the helix is very inferior to that of a steel magnet, or an electro-magnet. Still, with a delicate apparatus and a strong battery, a rapid revolution may be obtained. Theory also indicates that if the wire frame were suspended inside of a hollow helix, on a support erected for that purpose, it would still revolve. The manipulation of this experiment is not easy. It can be made to succeed by using a wire frame with only two arms, the extremities of which are sharpened to fine points and dip into very pure mercury.

Here followed a description and exhibition of the instruments. Figure 1, is where the frame is hung outside of the coil. Figure 2, is

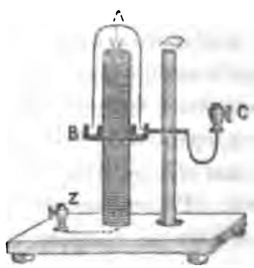


FIGURE 1.

the apparatus for showing the revolution inside of the coil. The frame is already hung upon its stand. When the experiment is tried, the coil (which is seen at H) is slipped over the frame and stand, and

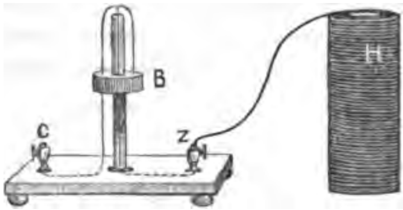


FIGURE 2.

rests on the base-board. The current enters at C, passes up a copper wire into the mercury basin at B, thence up the two branches of the movable frame, down the pivot and the metallic stand on which it rests, and by a wire underneath to Z. One extremity of the coil H is clamped to Z, and the other to the negative pole of the battery.

The failure of some skilful experimenters in obtaining a revolution, when the wire frame was hung upon the inside instead of the outside of the helix, has resulted from not observing the rule, so well understood when magnets are used, namely, of confining the action to a single extremity of the helix. It will be observed that the arrangement is, in principle, the same as in the experiment to show the revolution of mercury, the radial currents of which move by the forces exerted between them and the circular currents in the coil at the margin of the basin. But the motion is much more distinctly observed with the wire frame than with the mercury, and the rapidity of the motion is vastly superior to what can be produced with Savary's instrument, to which we have already alluded. Moreover, in this way of conducting the experiment, we are able to see at a glance the analogy between the forces here active and those which carry the frames around the poles of the magnet.

In the two experiments already described, in which we dispense altogether with the use of iron, it makes no essential difference whether the movable wire frame is inside or outside of the helix. It is far otherwise when we place iron inside of the helix, converting it into an electro-magnet. When the wire frame is suspended on the outside of one extremity of this electro-magnet, it revolves much more rapidly than if the helix alone, without the iron, were used. But when we introduce a hollow tube of sheet iron into the inside of the hollow helix, the wire frame will not move at all, though it turns

easily from the action of the same helix without the iron. The contrast here exhibited, surprising enough when first observed, is still just what might have been anticipated from the electro-dynamical theory of magnetism if applied to this particular case. This theory supposes, in brief, that magnetism in steel and iron originates in electrical currents, circulating round each particle, in planes at right angles to the

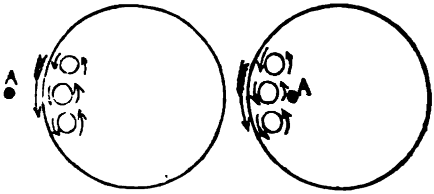


FIGURE 3. FIGURE 4.

magnetic axis. Let figures 3 and 4, represent horizontal sections of the two instruments, whose contrary action we wish to illustrate. The large circle is one strand of the helix. The small circles are the circulations around the particles of the iron. The arrows show the direction in which we suppose the currents to be flowing. As the currents of the iron are developed, that is, directed by that of the helix, their direction must be assumed such that, in the parts which are nearest to the helix, they may coincide with that of the battery current in this helix. The dot at A is a horizontal section of one arm of the revolving frame. If we examine figure 3, we shall see that the side of the small circle, which is nearest to the dot, is that which is also nearest to the helix; whereas, in figure 4, the side of the small circle which is nearest to the dot, is that which is farthest from the corresponding arc of the helix. Hence it follows, that, in the first case, the most efficient half of the magnet's currents, namely, the nearest half, will tend to turn the frame in the same direction as the helix moves it; while, in the second case, it tends to turn it in the contrary direction. Therefore, in one instrument the iron increases the velocity, and in the other it diminishes it. Any considerable diminution of the moving forces will destroy the motion entirely in so delicate an apparatus as that under consideration, unless proportionally stronger batteries are employed. It can easily be imagined that the direct action of the helix upon the frame, and its indirect action, as propagated through the iron, are exactly equal to each other in amount, though opposite in kind. If this were so, there would remain no residual power to move the frame, and no motion could be expected however much the strength of the battery current were increased.

Sixth Day, August 20, 1849.

SECTION OF GEOLOGY AND PALÆONTOLOGY.

REMARKS ON THE GEOLOGY, MINERALOGY, AND MINES OF LAKE SUPERIOR. BY C. T. JACKSON, U. S. Geologist.

THE short space of time that can now be devoted to communications made to this Association, warns me that it will be necessary to abridge very much the remarks which I am prepared to lay before you. I would willingly have refrained from any thing more than a few oral statements of the new facts discovered since I last addressed you on the Geology of Lake Superior, were it not necessary for the history of the explorations which have taken place, to give a short chronological account of what has been done by others in bringing to light the mineral resources of this remarkable region, and for my own protection, to recall to your memories the researches that I had made on Lake Superior anterior to my appointment to the office of United States Geologist.

The unfortunate relations which exist between myself and two of my late assistants, renders it expedient for me not to report on the districts that I assigned to them while in my employ, and hence I shall not represent any portions of those particular districts, but only those which were personally surveyed and explored by me. The unity of plan of my survey has been destroyed by interference, and since it is no fault of mine, I shall leave the responsibility to rest where it belongs.

It should be remembered that the value of the native copper mines of Lake Superior was first brought before the public by me, and that I took the responsibility (notwithstanding the opinions of many excellent geologists and miners were opposed to my views) to recommend mining operations on Kewenaw Point, and that I selected and pointed out the best localities for mining purposes. It is known, also, that the only mines thus far opened, in a proper manner, have shown that my predictions relative to these new mineral veins were well founded, and it is now ascertained that veins of native copper can be wrought profitably. Speculations follow for a while in the wake of all promising enterprises, and there are always pseudo-geologists to be found ready to sell reports that can be used in this business so ruinous

to the community and injurious to honest adventures. Lake Superior has had, as is well known, its speculation fever.

It is recorded in the relations of the French Jesuit fathers, who visited Lake Superior two hundred years ago, that the Indians obtained a great abundance of native copper from the lake shores, and from the beds of rivers that flow into the lake. The Indians regarded these lumps of copper as presents to them from the Miziszippi, the Great Spirit of the waters, who threw them up from the bottom of the Lake. They cherished them as holy things, and spoke of them with mysterious reverence.

The Jesuits nowhere describe veins of copper, or any metal, in place, and do not mention any proper mining operations as having been performed by the Indians; but it was discovered, in the diggings at Eagle River, in 1844, that the Aborigines had extracted the metal from the veins, and had made knives and spear-heads of the sheet copper which they obtained. This observation was very interesting to me, and, on searching at the other mines, I invariably found Indian stone-hammers, and proofs of superficial mining, by the native tribes. On calling the attention of the Directors of mines to these curiosities, they readily entered into the work of searching for them; and cart loads of Indian tools have already been excavated; and, at the North West Companies' mines, near Eagle Harbor, there was a depression of six feet, in the metaliferous lode, where the Indians had mined out the sheets of copper. Barrels full of hammers, much worn, were thrown out of this old excavation; and I have preserved a number of them for the Government collection. It is an error to suppose that any more civilized or superior race of people did this work, for the tools betray their true Chippeway origin, and are such as all Northern Indians made use of prior to the coming of Europeans.

I am perfectly convinced that most of the native copper veins now opened and wrought by European and American miners on Lake Superior, were known and worked superficially by the red men, hundreds if not thousands of years before America was discovered by Columbus.

It seems, however, that at the time of the early Jesuit missions of the French, in 1640, the natives had either ceased to work the metal from the veins in place, or that they concealed the fact from the Jesuit fathers. The latter is most probably the case, for they were, as the priests acknowledge, very unwilling to tell them where they obtained their native copper, and it is probable that they never did

confess to them the true localities of the copper lodes; for the "Relations," published by the missionaries, although they mention the abundance of native copper and the probability of their being good mines that might be profitably wrought, do not name any vein or mention any copper as seen in the rocks.

So soon as the white men furnished the Indians with steel tools and weapons, they abandoned the copper, and hence the localities were soon forgotten, and had to be re-discovered by modern enterprise. I have full details collected of all that was published by the Jesuits in their "Relacions," relating to the copper region, and shall give an abstract of them in my Report to the United States Government next winter.

Soon after the close of the French war, when Canada had been yielded to Great Britain, Alexander Henry, an enterprising English trader, set out on a voyage to Mackinaw, in 1760, and was taken a prisoner by the Indians on the capture of Fort Mackinaw. He was adopted as a brother by an Indian who rescued him, and travelled with him extensively on the shores of Lake Superior. His voyages extended from 1760 to 1776, and in 1809 his travels were printed in New York, in an octavo volume.

He describes numerous lumps of native copper which he saw on the Lake shores and in possession of the Indians, but he does not mention any locality where the metals occur in place. Mr. Norberg found a mass of ore that proved to be rich in silver, but it was a loose pebble on the beach of Point Aux Iroquois. I supposed at one time, from his description of this specimen, that it might have been a mass of chloride of silver, but am now disposed to believe it was native silver in the blue vein-stone, like that of the Copper Falls mine. Carver, in his travels, gives some account of native copper on the Lake shores, but his statements are loose, and want that truthful simplicity of Henry's.

In more modern times we have the observations of Henry R. Schoolcraft, who accompanied Gen. Cass in his travels on the Lake, and who visited the great block of native copper at Ontonagon, an account of which was published, with a plate, in the American Journal of Science, Vol. III. During the last war with England, or soon after, Dr. Francis Le Baron, of Plymouth, visited Lake Superior, and brought home a piece of the great copper rock of the Ontonagon. I remember very distinctly a large copper spoon that Dr. Le Baron had made of this specimen. It was for many years a curiosity in Mr. Tuft's

barber's shop, in Plymouth, and now is in the collection of Mr. Charles Cramer, of St. Petersburg, Russia.

The first proper scientific explorations in the mineral lands of Lake Superior, were made by my late friend, Dr. Douglas Houghton, while employed as the State Geologist of Michigan, and subsequently, while engaged in a connected Linear and Geological Survey, under the direction of the General Government. His publications were annual reports, in which he described the geology of the country, and the minerals he had discovered. He withheld any designations of localities of the metals and ores, giving only one locality, namely, that of the Greene Silicate of Copper, of Copper Harbor. There cannot be a doubt that he was aware of the localities of most of the exposed copper veins, for he had traversed the region where they occur, and was a good observer. It is supposed that he reserved a more full description of the ores and metals, with that of their localities, for his final report. This, unfortunately, he did not live to write. He perished in the field of his labors, on the coast of Kewenaw Point. It was my sad duty to announce his departure to my scientific brethren, and to pronounce his eulogy before this Association at its meeting in New York.

In 1843, General James Wilson obtained from the War Department a permit for a lease of a location three miles square, on the mineral lands of the United States in Michigan. By advice of a half-breed Indian, who was acquainted with the shores of Lake Superior, he took for his location a tract of land three miles square at the mouth of Eagle river. A company was organized, and a party of lead miners from Gratiot's Grove mines was sent, under the direction of Colonel Charles Gratiot, to explore for metals, and especially for copper.

The advice of Dr. Houghton was asked by the organizers of the Company, but he being in the Government employ, very properly declined giving his advice to a private company, and recommended them to employ me to examine their locations.

In 1844, I was employed by Hon. David Henshaw, to examine the above-mentioned location, and several others that had been secured; and in company with that gentleman I visited and carefully examined those locations, and selected those veins that appeared the most promising. Large quantities of every mineral were obtained, and assays were made of the ores on average samples. The results showed that mining could be advantageously carried on in that region, and that the metals could be easily separated. I surveyed ten veins of native cop-

per on lands belonging to the Company, and selected two veins as of sufficient promise for economical working. I examined also samples of ores brought in by the company's miners and explorers, and obtained information of most of the localities in the vicinity.

In 1845, I was employed to re-examine the mineral lands, and added much to our previous knowledge, surveying the veins belonging to the Pittsburg and Boston Copper Company, and particularly the Cliff mine, which is now so celebrated as a profitable native copper mine. This mine I shall presently describe as the model mine of the country, and as highly creditable to the faith and enterprise of its owners and to the indomitable perseverance and great skill of their miners.

In 1846, I did not visit the Lake, for speculations had already taken the place of wholesome business operations, and fancy stock was made and sold all over the country. Under such a state of things, any scientific man who valued his reputation, would naturally avoid entering into competition with quacks and fancy-stock report-makers. Instead of going to the Lake, I delivered a course of lectures on the business of mining, expressly for the purpose of checking this mania for stock gambling.

In the spring of 1847, by authority of an act of Congress, Hon. Robert J. Walker, Secretary of the Treasury, appointed me to make the Geological and Mineralogical Survey of the Mineral Lands of the United States, in Michigan, and I was called to Washington, received the appointment and the very full and minute instructions prepared for my guidance by the Secretary of the Treasury himself. These instructions I have carefully followed and should have entirely fulfilled if I had not been interrupted before the completion of the field work of the survey. This will render my report incomplete, for I have not at this time any authority to direct the finishing of the field work, and the survey I suppose is to pass from my hands. This is to be regretted, for it cannot have that completeness and unity of plan that it would have had under my immediate direction.

I feel it to be my duty to make this statement before this Scientific Association, and to suggest that interferences of the kind that have prevailed in obstructing the completion of my work, should be prevented, and scientific men, after the commencement of a public survey, should be assured of their rights, or they will not be willing to engage in the service of the Government.

On the Geological Structure of Keweenaw Point. By Dr. C. T. Jackson, U. S. Geologist.

This remarkable promontory extends from the south side of Lake Superior nearly into the middle of the Lake, from 46° 40′ to 47° 29′ north latitude, and is comprised between 87° 55′ and 89° 30′ west longitude. Its general direction is to the E. N. E., in the line of the trend of the great dikes or masses of Trap rocks which form its central ridges.

The surface of the country is broken and rolling, and some of the hills attain an elevation of nearly 900 feet above the lake level. Although but few species of rocks exist on this promontory, they present phenomena of remarkable scientific and practical interest. On the immediate coast, excepting at a few points, the first rocks that meet the eye of the geologist are a course conglomerate, made up of large rounded and smooth pebbles of red porphyry quartz, altered slate and sandstone, masses of Epidote rock, syenite and hard greenstone trap, mostly of the porphyritic variety, and regular strata of fine grained red and grey or mottled sandstone, devoid of any fossil contents.

The direction of the strata of sandstone and conglomerate is parallel to the line of uplift of the trap rocks, or E. N. E., W. S. W. Its dip is toward the W. N. W., at various angles, being greatest near the trap rocks, which come between their strata and divide the great masses of sandstone throughout the whole length of the promontory. The strata remote from the trap on Keweenaw Bay, and on the opposite side of the point, at the portage, are horizontal or but slightly waving, while near the trap rocks the dip of the strata is generally as high as 30°, and sometimes more. The conglomerate rock is limited to the borders of the trap range, and is of the same geological age as, the finer grained sandstones, and alternates with them.

At the line of junction of the trap rocks and sandstones, the sandstone and trap are interfused, producing that singular and very important metamorphic rock amygdaloid, a rock closely resembling the vesicular lava of volcanoes, but having its cavities filled with a great variety of curious and interesting minerals.

In the memoir published by Mr. Alger and myself on the mineralogy and geology of Nova Scotia, in the American Journal of Science, Vols. XIV., XV., 1828, will be found an account of the origin of amygdaloidal rocks, like those of Keweenaw Point, and it may not be

uninteresting to compare the trappean ranges on Lake Superior with those of Nova Scotia. On inspection of the map it will be seen that the great trappean band on Keweenaw Point is parallel with that on the borders of the Bay of Fundy in Nova Scotia, and it will be further noticed on examination of the geology of these distant regions, the conditions of the rocks are similar, if not identical. The trap of Nova Scotia, like that on Lake Superior, protrudes from below the red sand stone, supposed to be the new red, and passes between the strata in the line of least resistance. Amygdaloid, with species of minerals similar to those of Lake Superior, excepting Prehnite, which is rarely found in Nova Scotia, exist also at the line of junction of the trap rocks and sandstones of Nova Scotia. Native copper occurs in the amygdaloid of both places, but is more commonly found in the trap tuff of Nova Scotia, while it occurs more abundantly in the amygdaloid of Lake Superior. Heulandite is rare in the Lake Superior trap rocks, while it is extremely abundant in Nova Scotia, but the other minerals are of the same species in both places.

It will be observed on examination of the geological maps, that the same gentle crescentic curving of the trap bands towards the north-west was noticed in both countries, a fact also recorded by Profs. Percival and Rogers, in their reports on the geology of Connecticut and New Jersey.

General geological laws seem to have prevailed in all the regions where trap rocks have burst through sandstone, the effects of heat being recognizable, and proportional to the relative masses of intruded rocks.

It cannot fail to strike every geologist familiar with rocks of igneous origin, and their effects on sedimentary strata, that the history of the origin of trap rocks is indelibly recorded, and that they are really lavas that have risen from the interior of the globe through fractures in its crust, taking the line of least resistance by passing between the strata.

By the influence of heat the sedimentary strata were interfused with the igneous rocks, and it is a singular fact that amygdaloid is most abundantly produced by the action of trap rocks on sandstone, and that copper is the most usual metal found in the fissures, amygdules, and pockets of the resulting amygdaloid.

True workable veins of native copper in this class of rocks had not been described, so far as I know, anterior to my researches on Lake Superior, and it was regarded as contrary to all experience that this

metal should thus occur in quantities sufficient for profitable mining. The only locality where native copper has been mined to any extent, is in Siberia, but the metal is not in trap.

Having satisfied myself of the fact that adequate quantities of the metal did exist in veins in the amygdaloid trap of Lake Superior, I ventured to recommend the opening of mines on Keweenaw Point, on and near Eagle River, and the result has proved that native copper veins can be profitably wrought. I mention this fact now in order to recall to your minds the objections that were made to my views on this subject at our meeting in New Haven, in 1845. The predictions I then made are now fully verified.

NATURE OF THE VEINS IN THE TRAP ROCKS.

There are two classes of veins known to miners on Lake Superior, viz.: 1st. Those running with the "country," or parallel to the course of stratified rocks through which the trap rocks pass — veins that are sometimes called beds, or interstratified masses. And 2d. Those which cross the "country," or cut transversely at various angles the line of direction of the strata.

These last are called true veins, and are the only ones on which miners have thus far placed reliance as to their continuing rich to any considerable depth. I do not regard the question as fully settled by experience in this district, that mining should be confined to the transverse veins, for there is reason to believe that both classes of veins are of the same origin, and no facts have yet been adduced to prove that veins running with the "country" cannot be advantageously wrought. On the contrary, it is known that large quantities of native copper are raised from this class of veins on the Ontonagon River, and it is probable that some on Isle Royale will ultimately prove valuable. A few good practical experiments in mining will settle this mooted point in practical geology. It is obvious, since the trap rocks are not really stratified, that this class of veins cannot be correctly denominated interstratified, though they may be imbedded.

The first class of veins run, as will be understood by what I have previously said, nearly E N.E. W.S.W., varying with the flexures of the line of junction of the trap and sandstone, and are included between the two rocks in amygdaloid or in epidote, this mineral being the most usual gangue or matrix of the copper. Regular walls of solid copper of some inches in thickness, have been observed in one of the new mines opened in the Ontonagon River, and sheets of considerable size have been found in the east and west veins on Isle Royale.

Mining operations are now in progress to test the permanency of these veins, we shall know in a year or two the results.

The second class or transverse veins run generally in a course N. 26° to 30° W., S. 26° to 30° E., and consequently cut across the line of direction of the trap rocks and adjacent strata. They are especially rich in the amygdaloidal trap, and thus far have not been profitably worked beyond its limits. In the hard trap rock they are pinched or become narrow, thin plates of metallic copper filling these seams in the trap. The "veinstone" contains the following species of minerals: Prehnite, Calc. Spar, Laumonite, Leonhardite, Quartz, Datholite, Chabasie, Mesotype, Apophyllite, Feldspar, Analcime, and Wollastonite.

The most common veinstone is Prehnite, which occurs in regular symmetrical veins, the Prehnite encrusting the sides of the fissures and closing in the middle of it by crystallized botryoidal surfaces. At the surface these veins are rarely more than six inches in width and containing only minute scales of metallic copper, the presence of which in decomposed veins is most readily detected by spots of green carbonate of copper, derived from exposure of the metal to the air and water. These narrow and poor veins of Prehnite enrich in copper as they descend into the rock, until at last the Prehnite gives way to copper and its space is entirely occupied by it, a thick vein of solid copper filling the fissure, while the Prehnite was either absorbed by the rock, or the condition of the rock was such that it could not be formed.

At the Cliff Mine of the Boston and Pittsburg Mining Company, the vein at the top of the cliff consisted of Prehnite, containing only minute scales of copper, and was only six inches wide, but it was found on descending that this vein widened, about two hundred feet lower down, to eighteen inches, and lower still it had widened to two feet, and was charged with from five to thirty per cent. of metallic copper, and some particles of silver. The average yield of a large sample of the vein at the surface was found to be 5 6-10th per cent. of copper, and it was estimated that the ore could be practically "bucked" or picked up to 15 per cent.

The width of the vein was estimated to be three feet at the base of the hill, where it was still concealed from view by the soil. On driving a level into the cliff and one at the base of the hill, the vein was proved to be much richer than at the surface, and on sinking a shaft to the depth of 226 feet below the base of the hill, it exposed sheets

of copper varying in thickness from a few inches to three feet. These masses of copper filled the vein, and the Prehnite and other zeolitic minerals disappeared. By carrying forward levels at the proper points, sixty feet below each other, and by stopeing out the backs of the levels, large flattened elipsoidal masses of copper were exposed, and removed by heavy blasts of gunpowder. These masses were then cut up by mortising out channels through them by means of steel chisels, driven by a heavy sledge hammer.

Some idea may be formed of the rapid increase in richness of this lode by comparing the poor Prehnite vein at the top of the hill with the ponderous masses of pure copper that are now cut up in the mine below. One mass of pure copper extracted while I was on the survey, weighed eighty tons, and other masses probably of equal magnitude were in process of being uncovered.

Taking into account the height of the cliff in which the vein is seen, and the depth of the shafts at its base, we have the vein proved 526 feet deep, and thus far it has been steadily enriching, and has surpassed the most sanguine expectations of all the miners and geologists who had examined it.

Already this mine sends to market nearly a thousand tons of copper ore per annum, the ore being estimated to contain sixty per cent. of pure copper after it is cleansed of the adhering rock. This mine, it is understood, has paid for itself and made a dividend of ten dollars per share to its owners.

It is highly probable that other mines on Keweenaw Point, if wrought with the same energy and skill, would prove equally valuable, but thus far no mining equivalent to that of the Boston and Pittsburg Mining Company has been attempted, and it is difficult to find a miner so competent to the task as Capt. Jennings, the Cornish miner, who has had charge of this remarkable mine. I exhibit to you a profile and plan of the mine, in which all the excavations are fully delineated.

Among other promising mines are the North American, the Copper Falls, the North-west, and the Phenix, all of which have been sufficiently proved to warrant the belief that they can be advantageously wrought, but still it must be remembered that even in the best known mineral districts, mines frequently fail to prove profitable from causes that are not at once foreseen. The North American Company's mines are situated very near the Cliff mine, on the west branch of Eagle River, and are now wrought with energy, and give promise of

success nearly equal to that of the Cliff mine before described. The veins are similar in their nature and in their contents, so that I need not describe them.

The Copper Falls mines have been opened to a considerable extent, and from one of the veins a single mass of copper was taken that weighed eight tons. It was sawed into pieces and sent to market. I exhibit to the section a specimen sawed from this mass. It is perfectly pure copper, and as dense as the purest hammered copper of commerce, showing its perfect fineness. There is a considerable proportion of native silver mixed with the copper of this mine, and in the green veinstone, a specimen of which I lay before you. Silver is found also in most of the copper mines of the lake, and frequently in sufficient quantities to be of commercial value. It is most curiously united with the copper, and in some of the pieces I lay before you, the metallic copper is actually porphyritic with masses of silver, and yet the silver is absolutely pure, and the copper is also pure, there being no alloying or chemical union, but a mere metallic cementation at the line of contact. This phenomenon is seen in all the localities on the lake where native copper and silver occur together, and this state of the metals must have arisen from a common cause acting in every one of the veins. It is not capable of being explained in the present state of chemical and geological knowledge, and is a subject for experimental research. The solution of this question will lead to an explanation of the origin of the native copper and silver veins, the rationale of which we have not yet reached.

All the experiments I have devised and executed, to discover the cause of the separation of the copper and silver, as seen in these specimens, have given negative results. The experiments were made on fused alloys of silver and copper. The metals did not separate by galvanic agency. Little has yet been done towards extensive working of the North-west Company's copper mine. It is situated a few miles from Eagle Harbor, and several rich veins of native copper, with some native silver, have been discovered and wrought to a small depth. The want of confidence in this new kind of mining prevents the investment of sufficient capital and the employment of a sufficient mining force to work the mines. I am confident, that, with capital and skill, this company's mine might soon be rendered profitable.

The old Lake Superior Company, the first organized for mining on Lake Superior, was unsuccessful in its first operations from several causes, among which the want of miners capable of carrying on the

work in a proper manner was the chief; and it should be remembered that mining for native copper was, at that time, a new business to both English and American miners. Numerous changes were made and many new shafts were sunk, but no regular preconceived plan was ever carried out, and hence the mines are in a neglected state. Much allowance should be made for the newness of the enterprise, the wilderness state of the country, and want of skill in the miners hastily collected from districts wholly unlike the one they were sent to explore. There was also an erroneous opinion prevalent among many of the original stockholders that mining could be made profitable from the outset, — a most fallacious idea. The company soon closed up its mining operations, and I have heard that a new organization has been since adopted, and it is hoped, if mining operations are again begun, a regular system will be pursued, modelled after the plan of the Boston and Pittsburg Company's workings. The Lake Superior or Phenix Company's veins are rich in native copper and silver, and although the leader or Prehnite vein is but a few inches wide, it will doubtless lead to a solid copper vein, like those heretofore described. Masses of pure copper, of large size, weighing some thousands of pounds, were obtained from an ancient ravine or excavation that had been worn out by the river running over the vein, and large pieces of silver were also found. These show the contents of the lode in the true vein. Most of the work heretofore executed at this mine has been done in the western wall of the vein and not in the vein itself. To the company owning the Lake Superior mine is due the credit of the earliest mining enterprise on the Lake, and those who have followed after them should remember that they opened the way and introduced the business of mining into the then unbroken wilderness of Lake Superior.

It is still a question among geologists and miners whether veins were filled by igneous injection or sublimation, or by aqueous and galvanic deposition. This question is one of very great scientific and practical interest, and is exceedingly difficult to answer so far as relates to the native copper and native silver of Lake Superior.

The objections to the igneous origin of native copper are, 1st, that the metal bears the imprint of crystals of Prehnite, as seen in the specimen I lay before the section, and we cannot account for the fact that this zeolite was not rendered anhydrous by the molten copper. 2dly, that if the copper was melted, since its fusing point is much higher than that of silver, that the silver is not alloyed with the copper,

but is separate from even a trace of it in chemical combination, though small particles and large lumps of silver are mixed and united with the metallic copper.

These objections are equally strong against the theory of sublimation of the copper, and since silver is not volatile at the highest temperature of our furnaces we could not account for the presence of that metal by a simultaneous sublimation of the metals.

Against the theory of its aqueous deposition, or its origin from any solution of copper, it may be urged that if the metal was in chemical solution, no material capable of causing its decomposition with the deposition of the copper in a metallic state exists in the vein, and no salt, if any supposed acid solvent, which would result from the decomposition of its combination, exists in the vein. Again, it would be impossible for the chasm to contain a sufficiency of any copper solution, however concentrated, to produce the solid metallic copper filling the fissure, for, as before observed, the masses of copper are from a foot to three feet in thickness, and occupy the whole space of the sundered rock.

Galvanic segregation it has been supposed would explain the origin of these copper veins. But we may ask, from what was the copper segregated? It is impossible for galvanism to create the metal from the ingredients of trap rocks, or sandstone; and we can hardly imagine any arrangement of the rocks that would produce a galvanic battery with its poles so arranged as to effect the deposition of a vein of solid copper two or three feet in thickness.

It is well known that the trap rocks are magnetic, and that they possess polarity at the surfaces of disjunction. This has been fully substantiated by the researches of Dr. Locke and others on the Lake Superior mineral lands, but this magnetism is obviously the effect of the earth's inductive magnetism exerted on the very large proportion of magnetic iron ore entering into the composition of the trap rocks, a quantity so large that I have seen pig iron made directly from those rocks by fusion in a blast furnace, about twelve per cent. of iron being reduced from it; and we know, from the experiments of Dr. Locke, that even fragments of the trap rock are both magnetic and polar. It remains to be proved, that there are any *electric* currents in the native copper veins, for such currents are by no means proved by deviations of the magnetic needle, which are doubtless produced by the magnetic polarity of the trap rock itself.

The occurrence of bright scales and perfect crystals of native

copper in perfect crystals of prehnite, datholite, calc-spar, and quartz would seem to indicate a simultaneous deposition of the copper and those crystalized minerals including it, or that they were impregnated with native copper by sublimation immediately before the injection of the principal copper vein took place. If we could admit the igneous formation of zeolites, and of calc-spar, there would be less difficulty in accounting for the veins by sublimation or injection, or by both methods, but this chemists will not readily admit, for the zeolitic minerals are generally hydrous, the occurrence of anhydrous Prehnite or "Jacksonite" being the only exception thus far determined.

It is also a question whether the native copper in the amygdaloid was derived from the interfused sandstone, or was mechanically brought up with the trap rock. It has been imagined, that since the sandstone is made up of the detritus of more ancient rocks which might have contained copper ores, that the copper ore being deposited with the sand was reduced by the action of the trap. This idea would be plausible, if it could be shown that the sandstone in the vicinity of the trap contained copper in a sufficiency to account for that in the amygdaloid; but such is not the case. It has been said that local deposits of the ore might have taken place in portions of the sandstone strata, and that the trap rocks came up and reduced it. This would be imputing a most remarkable degree of intelligence to the trap rocks, that they should know exactly where the copper ore was deposited, and come up at those places expressly to smelt it! I must confess that I cannot attribute the origin of the copper to any other causes than those which produced the trap rocks themselves, and that the copper came from the molten interior of the earth seems, at least from what we know of igneous agencies, to be most probable. There are veins in the conglomerate rocks which are filled with calcareous spar, containing crystals of copper, some of which will weigh half a pound, and are generally in the rhombic dodecahedral form. One of the calc-spar veins at Agate Harbor has yielded masses of copper weighing several hundred pounds.

At Copper Harbor, a large vein of solid black oxide of copper was found in the conglomerate rock. This ore is not known to exist in any considerable quantity elsewhere. The ore in the vein was fourteen inches wide, and for a short time the mine furnished a good supply of copper ore, yielding about sixty or seventy per cent. of metallic copper. It was soon exhausted, a bed of fine grained sandstone cutting off the copper vein, the calc-spar only continuing in the

sandstone below. Among the masses of black oxide of copper brought from the mine at Copper Harbor, Mr. J. E. Teschemacher discovered regular cubic crystals of the ore, crystals which show that the ore is not a mere mechanical mixture of copper smut with earthy matters for a cement, as some have supposed. A pure specimen analyzed in my laboratory yielded 79.86 per cent. of copper.

There are also found at the Copper Harbor mine, chrysocolla, or hydrous green silicate of copper, and the black silicate, which contains a less proportion of water. These ores, we can easily conceive, might be produced by the decomposition of a solution of copper by the action of a hot solution of lime. The black oxide may have been derived either from a solution, or from igneous sublimation. We know that black oxide of copper is sublimed from the crater of Vesuvius, and is deposited in fine splendent scales like specular iron ore in the lavas.

Chloride of copper is very volatile, and is sublimed in the crater of Vesuvius. It is also known to be volatilized in the blast furnace. The experiments of Mr. Frederick W. Davis, at the Point Shirley copper works, having fully demonstrated the fact that a considerable proportion of copper is lost by sublimation from copper ores, containing the chloride of that metal.

These facts may at some future day serve to explain some of the phenomena relating to the formation of metallic veins. At present there is no part of geological science so little understood as the theory of veins, and on this account I am desirous of calling the attention of the Section to this subject.

With respect to the age of the red sandstone of Lake Superior, I would remark that there have been and still are differences of opinion. No distinct fossils having been found in it; the usual index for fixing the geological age of stratified rocks is wanting.

From the mineralogical character and the geological associations of the rocks, their parallelism to those of Nova Scotia, and their mineral contents, I was led in 1844, to suggest the identity of the two formations, as contemporaneous, and regarded the Lake Superior sandstone as the new red, or at least as of the same age with that of Nova Scotia, New Jersey, and Connecticut. This idea I still favor.

During the last year the linear surveyors, who were engaged in subdividing townships on the southern portion of Keweenaw Point, discovered a large protruding mass of Silurian limestone, around which the sandstone strata are horizontal. My assistant was sent to

examine this limestone, and states that its strata lines dip about 30°. A fragment of a fossil, probably a pentamerus, was also found in the limestone. These facts would seem to prove that the sandstone is above the silurean limestone, and consequently that it is either the old or the new red. The absence of fossil shells in the sandstone would lead us to conclude that it does not belong to the old red, and consequently we are led back to my original opinion, as published in the American Journal of Science, in 1845, that the Lake Superior sandstone is of a later date, and is probably the new red.

This opinion was also expressed by Monsieur De Verneuil, during his visit to the Lake in 1846, but I do not know from what data his opinion was formed. It has been asserted that the Lake Superior sandstones pass beneath the Silurian rocks, but I do not think the fact has ever been observed.

ISLE ROYALE.

This Island is situated on the north side of Lake Superior, in Latitude 48° North, Longitude 89° West. It is about forty miles in length, and five or six miles wide. It presents a broken and rugged outline on its coast and is deeply indented by long and narrow inlets and bays, all of which are parallel to the ranges of the trap rocks which constitute the ridges traversing the island throughout its length. Several small lakes are also seen lying between the trappean hills and coinciding with their line of bearing.

The general direction of this island is parallel to that of Keweenaw Point, and the trap rocks are of the same geological age and have uplifted the sandstones of the same epoch.

On the South-western end of the Island the fine red sandstone strata are seen near Card's Point, and they extend along the coast of Siskawit Bay to Epidote Cove, forming gently sloping sheets extending out into the lake to a considerable distance. Conglomerate rocks border the coast nearly to Rock Harbor and lie next to the trap. The inland boundary of the sandstone was ascertained by my sub-agents to be parallel to the coast line where it is exposed. About one fourth the area of the Island is sandstone and conglomerate rock. All the rest of it consists of trap, which forms ridges attaining an elevation of from three hundred to five hundred feet above the lake, and extending in a broken line throughout the whole extent of the island. In some places isolated masses of trap rocks form tall towers standing like high chimneys on the hill sides; in others, picturesque islands covered

with dark spruce trees are seen jutting out on the coast, or standing like watch towers at the entrance of the harbors.

Isle Royale was better known to the Indians as a good place for catching Siskawit than as a mining region; and it is probable that the name *Menung*, signifying a good place, refers to the fisheries, but it is certain from the "Relacions" of the Jesuit fathers that they were aware of the existence of an abundance of copper boulders upon its shores.

Numerous explorers had visited Isle Royale anterior to my survey, but mining operations had not been entered upon to any extent on account of the difficulties arising from some official misunderstanding as to permits for leases. Dr. Locke had selected some veins and beds of copper for the Ohio and Isle Royale Company, and explorations were going on to determine the probable value of several veins.

There are two kinds of veins on this island, as before mentioned. The widest are those near Rock Harbor. They are thick beds of solid epidote rock filled with small spiculæ of copper, there being from eight to ten per cent. of the metal in the gangue. These beds dip but slightly from the horizon, rarely more than 15 or 20°, and crop out on the south side of the island a few feet above the surface of the Lake. Beneath the copper-bearing bed of epidote, which is a foot in thickness, is a large bed of barren epidote rock, six feet thick, and very hard. Trap rocks overlie the whole, forming bold precipitous shores. No mining operations have yet proved the extent of these cupriferous epidote rocks, but they are exposed to a sufficient extent to render it probable that they will prove of value.

Another set of true veins occur, cutting nearly at right angles the trap rocks, and traversing the country. These veins are generally narrow, and are filled with Datholite, Prehnite, and native copper. The Datholite is very abundant, and may prove of economical importance either as a flux for copper ores, or as a material suitable for the munufacture of borax. One of the locations of the Ohio and Isle Royale Company was named, by Mr. J. H. Blake, Datholite, on account of the abundance of that mineral in the veins of copper.

At Todd's Harbor mines have been opened, and a considerable quantity of native copper has been obtained by Mr. McCulloch. Other veins have been opened at Scovill's Point, but as yet none of the veins on the island have been sufficiently proved to authorize the erection of permanent works for mining and smelting. One vein of each kind, opened to a considerable depth, would give much valuable

information concerning the permanency of the veins, and determine whether they widen and enrich or not.

It is extremely difficult for any one to decide absolutely on the value of a metalliferous vein, and it is only possible to form an approximate estimate where all the conditions of the problem are capable of being determined, and it is rarely the case that we have any thing more than a superficial view of the contents of a vein. It has been proved by a writer in the French Annales des Mines, that in Germany and France only one twentieth of the mines surveyed and recommended by the Royal Engineers of mines have paid a profit to the Stockholders, hence we should remind persons about to engage in mining adventures that their chances of success are only about five per cent.

When a mine is well proved it generally holds good veins, rarely running out in depth unless the rock changes, and then the vein generally alters also.

How far the native copper of the Lake Superior mines continues in depth is yet unknown, but the veins if they traverse sandstone strata will certainly change in that rock, and experience has thus far shown that the copper diminishes in that portion of the vein which traverses the sandstone. This has been fully proved at the Copper Falls mines, where a bed of sandstone, seventy-two feet in thickness, has changed the character of the lode where the vein passed through it, calc-spar filling the chasm and the copper nearly disappearing in the veinstone.

It was hoped that the vein would enrich after it had passed through the sandstone into the nether bed of trap rock, but it was found to be diffused into string veins of little practical importance. Owing to the limited extent of the amygdaloidal trap the true or transverse veins are not of great length, two thousand feet being perhaps an approximation to their linear extent, though it is possible that some may be longer. The idea of tracing a vein by its course over an extensive tract of country has proved fallacious on Lake Superior, and only the geological character of the country can be relied upon as a tolerably correct guide. The river beds, depressions in the soil, corresponding to the usual direction of the veins, afford the best facilities for finding veins, and by means of the solar compass and magnetic needle, lines of contact of the sandstone and trap may be readily found, and the amygdaloid is formed at those junctions. It was observed in my first visit to the Lake, that the productive copper veins occur where there are the greatest number of alternations of sandstone and trap rocks, as

shown by the diagram exhibited to the Section. No, less than six alterations of these rocks were observed near Copper Falls and Lake Superior mine in my surveys during the summers of 1844 and 1845. Subsequent researches have confirmed this observation.

From these data we should expect copper veins at the line of contact of the sandstone and trap on Isle Royale, but thus far in only a few places have the rocks been uncovered in the vicinity of the junction — some loose masses of native copper found on the shore of Siskawit Lake and the veins at Datholite Cove being the only facts obtained in conformation of this opinion.

In the hard columnar and compact trap there is little hope of finding valuable veins, for only narrow and tightly pinched seams of copper have thus far been found in these rocks.

On the Ontonagon river there are several veins of copper that run with the "country." They are now in course of trial, and it will soon be ascertained whether they can be profitably worked or not. The opinion of a practical miner, on whom I place reliance, is favorable to some of these mines. It was my intention to have examined them myself, this summer, before drawing up my report, but it has been ordered otherwise.

Rich ores of iron have been found in inexhaustible quantities in the district of country extending from the Menomonee River to Dead River. I have not had an opportunity of examining the localities myself, but I had obtained rich specimens of the ore from the Menomonee river, in 1844, through the agency of M. Barbeau, who obtained them from the Indians, and in 1845, the Indian chief who furnished those specimens, guided Mr. Pray to the Iron Mountain near the Menomonee River. During the past summer this locality has been also examined by one of my assistants.

On the Mineral Region of Lake Superior. By James S. Hodge.

Mr. Hodge began by remarking that as considerable time had already been devoted to this subject, his observations would be much curtailed from what he had prepared, and he would only cursorily touch upon the prominent points of the several divisions of the subjects. These are,

The extent of the present Actual Mining Region;

The number and character of the Productive Mines;

Some Peculiar Features in the relation of the Veins to their Repositories ; and

The Relics of Ancient Mining Operations found throughout the Country.

The general range and character of the metalliferous amygdaloid has been too often described to require of me a very particular notice. The outcrop of that belt of the rock, hitherto found productive, occupies a narrow strip of country along the southern base of the trap ridge, which, on Keweenaw Point, lies a little back from the south shore of the lake, and reaches farther and farther inland towards the southwest, till, beyond the Ontonagon River, it is more than twenty miles in the interior. The length on which this range is worked at intervals is about one hundred miles. Its features on Keweenaw Point differ somewhat from those near the Ontonagon. On the Point, the productive portion so far proves to be a belt of amygdaloid of unknown width lying beneath the mass of compact trap, which forms the summit of the ridge. There is no positive evidence that the amygdaloid exceeds a few rods in thickness without interruption, though it may attain much greater dimensions. Its thickness is important, because the veins which pass vertically across these rocks, at right angles to their course and to the lake shore, are only productive in the amygdaloid ; and as this rock passes under the compact trap of the steep ridge by a northerly dip of about 20°, it would soon carry the rich portion of the vein out of reach, unless the rock attained a considerable depth or thickness, as well as lateral extension. The law elsewhere established of veins that are productive in one rock ceasing to be so in passing into another, is nowhere more strikingly displayed than here. The *thickness* of the amygdaloid, then, here determines the profitable length of the veins. But in the vicinity of the Ontonagon River the veins pursue the same course with the belts of rock, and are therefore retained for indefinite distances in the same band.

On Keweenaw Point the ridge presents a long gentle slope towards the Lake ; but on its south side abrupt precipices of greenstone trap overhang the plain and swamp below, on whose margin next the cliff the amygdaloid crops out.

Although on the north slope the rock is sometimes porphyritic, which in other mining regions is regarded as a favorable character for the existence of rich veins ; and although occasional bands of amygdaloid alternate with the compact trap, no profitable veins have yet been opened in this position. If on Keweenaw Point they have been found

of promising appearance, the bands of this rock have generally proved too narrow for the veins to attain much length and depth before passing into the adjoining compact trap.

A little to the south of this ridge is another parallel range of hills, in which porphyritic rocks prevail. In it the metallic veins consist of the sulphurets of copper,—the grey and vitreous copper composing the principal metalliferous parts of the lodes so far as they have been opened. The productiveness and value of these mines is yet questionable.

Farther up the Lake are the trap ranges of the Porcupine mountains. No productive mines have been opened in them, though many veins have been partially wrought. This portion of the mining region is not yet proved, and no operations are now in progress tending to determine its importance.

My remarks, therefore, *upon the number and character of the productive mines*, will be limited to those scattered along the narrow metalliferous strip extending from Keweenaw Point southwestwardly across the Ontonagon.

It is not my intention to describe each of the productive mines on the south shore. An account of them is in course of publication in the pages of the American Railroad Journal. The names of those proved to be valuable are the *North West*, *Cliff*, *North American*, and *Minesota*. Other mines similarly situated, and presenting equally favorable external features, and which have already furnished some copper, are the *North Western*, *Albion*, *Douglass*, *Houghton*, *Algonquin*, and *Ontonagon* (*of Boston*.) These are at present lying idle only for want of the necessary capital to work them.

The lodes consist of veinstones, as quartz, laumonite, calc-spar, prehnite, epidote, and chlorite, with fragments of the wall-rocks. Native copper in fine particles, lumps, and sheets, are interspersed through these gangues, and huge masses of the metal lie longitudinally along the course of the lodes. Their shape is that of ellipsoidal sheets; and they stand on their edges, sometimes several arranged side by side. Native copper also penetrates the wall-rocks forming "*stock works*," which have sometimes led to extensive explorations entirely off the veins. On one side or the other the vein cleaves perfectly from the wall; on one side it often clings to it.

The mines are wrought wholly for native copper. The veinstone with scattered particles, furnish what is called *stamp work*, which is crushed under heavy stamps and then washed; the lumps are called

barrel ore, being packed in barrels for transportation; and the masses after being cut up into pieces not exceeding two tons in weight, are shipped in bulk. The size of some of these masses is so enormous as almost to exceed belief. They have been broken up in the Cliff mine of sixty and even eighty tons in weight. Such pieces are reduced in the mine to fragments of seven tons weight and less, and after being hoisted to the surface are still farther reduced.

At the Minesota Mine, near the Ontonagon River, I had an opportunity of examining in June the most extraordinary mass yet met with. Two shafts had been sunk on the line of the vein one hundred and fifty feet apart. At the depth of about thirty feet they struck massive copper, which lay in a huge sheet with the same underlay as that of the vein — about 55° towards the north. Leaving this sheet as a hanging wall, a level was run under it connecting the two shafts. For this whole distance of one hundred and fifty feet the mass appears to be continuous, and how much further it goes on the line of the vein either way there is no evidence, nor beside to what depth it penetrates in the solid vein. I examined it with care, striking it repeatedly with my hammer in order to detect if possible by the sound any break or interruption there might be in the mass — for a thin scale of stone encrusted it sometimes and concealed the face of the metal. Examinations had been made by drilling through this scale, where it attained the thickness of an inch or so; but in no place had any sign of a break been found. It formed the whole hanging wall of the level, showing a width of at least eight feet above the floor in which its lower edge was lost. It had been cut through in only one place, where a partial break afforded a convenient opportunity. Measuring the thickness here as well as the irregular shape of the gap admitted, it was found somewhat to exceed five feet. Assuming the thickness to *average* only one foot, there would be in this mass twelve hundred cubic feet, or about two hundred and fifty tons — still it is not safe to assume even one foot, for the masses vary extremely in thickness.

The mode adopted to remove these masses is to cut channels through them with cold chisels, after they are shattered by large sand blasts put in behind them. Grooves are cut with the chisels across their smallest places, one man holding, and another striking, as in drilling. A chip of copper three fourths of an inch wide and up to six inches in length is taken out, and the process is repeated until the groove passes through the mass. The expense of this work is from eight dollars to twelve dollars per superficial foot of the face exposed.

Fragments of veinstone enclosed in the copper prevent the use of saws. A powerful machine, occupying little room, is much needed, which would perform more economically this work.

The greatest thickness of any mass cut through at the Cliff Mine has been about three feet. Their occurrence through the vein is not regular. Barren spots alternate with productive portions. The same is the case in all the mines. The total product of the Cliff Mine for the year 1848 is estimated at eight hundred and thirty tons, averaging sixty per cent. During the present year, more than half this amount has been already sent down, and there is enough more on the surface and in sight in the mine to warrant the belief that one thousand tons will be the product of the year's work, or six hundred tons of copper. The whole amount of copper annually imported into the United States is about the value of $2,000,000, or about five thousand four hundred tons. But little has been supplied from our own mines. Nine such mines then as the Cliff would render us independent of foreign supplies. From present appearances, after careful examination of the region, and consideration of the progress made in mining since my last visit in 1846, I feel myself warranted in expressing a decided conviction that this amount of copper must be supplied in very few years, and this metal soon become, as lead already has, one of export instead of import. The recent failures of mining speculations wildly undertaken, and ignorantly and extravagantly conducted, may for a time check the development of these mines; but their wonderfully rich character is now beginning to be properly appreciated, as well as the reliance, which may be put in the surface-appearance of the veins. Some curious features in their character and distribution have been detected, which have heretofore escaped observation for want of sufficient data, and which will, I believe, be found of great consequence in the selection of the best localities. These after farther examination, I may at another time make public. The history of these mines so far has remarkably proved the foresight and excellent judgment of the lamented Dr. Houghton, particularly so in his predictions of the disastrous effects that must result from such speculations, as have caused the country to be overrun by hordes of adventurers.

The silver found associated with the copper has not proved of much importance, perhaps for the reason that the greater part of it is purloined by the miners. The Cliff Mine has probably yielded more than thirty thousand dollars' worth, of which not more than a tenth part

has been secured by the proprietors. I saw myself, the present season, no less than six pounds and eight ounces of lumps and bars of silver seized in the hands of an absconding workman.

As to the structure of the veins, remarks have already been offered by Dr. Jackson, and hypotheses suggested as to the various modes by which such immense masses of copper have been brought into the vein-fissures. Objections were offered as to the probability of their being due to igneous injection, for the reason that the copper was impressed with the forms of crystals of quartz and prehnite of the veinstones. May not the fissures have been filled by igneous injection, and the sublimation from these masses of molten matter gathered around the crystalline forms in the veins, and so taken their casts? As the ragged masses of copper contracted on cooling, clefts and cavities were left, into which subsequent argentiferous injections may have penetrated, the silver occupying the cavities as moulds; and failing to unite with the copper for want of sufficient heat to melt the latter. The two metals, as they are found closely connected together, yet each distinct from the other, may be imperfectly separated by a low heat, just sufficient to melt the more fusible silver, leaving the copper behind.

The point to which I wish to ask attention, as to the relation of the veins to their repositories, is the different character of the two sets of veins — that on Keweenaw Point, and that in the vicinity of the Ontonagon. The former, as stated by Dr. Jackson, crosses the range of hills nearly at right angles; but the latter, as I have observed, pursue a course longitudinally with the ridges, and are included in the bands (which for convenience may be called *strata*) of the amygdaloid. They have the same underlay or dip, as well as bearing of the trap; the Minesota vein, for instance, lying nearly east and west, and dipping about 55° towards the north. The former set are evidently true veins; but about the latter a question has been raised, whether they were not rather to be regarded as included beds, and therefore not permanent. The same question has been raised as to the character of the veins of magnetic iron ore of the Champlain district of New York, and of those of New Jersey, as well as of the zinc mines of the same district. And it is even understood that an unfavorable report has been presented of these zinc mines, by a distinguished French geologist, on the ground, that being included beds in the strata of gneiss, they cannot be depended upon for permanent supplies of ore. The subject is a highly important one, being so closely

connected with the mining interests of the country. And if such opinions are to be admitted, they cannot fail to effect the value of many of its at present most highly estimated mines. From many facts connected with these metalliferous deposits, I cannot but regard them as of the same general character with veins which cross the strata. Like them they are filled with similar vein-stones and metals, their relation to the wall-rocks is the same — one side often clinging to and passing into the wall, and one side cleaving, in a strongly marked line of separation, from it. The metals and ores are disseminated, more or less, through the wall-rocks; and it is hardly to be conceived, that because the one set happened to pass along these lines of separation of the rocks, which were the lines of least resistance, they should be regarded as of uncertain character; while to the other set, which cut across the strata, an entirely different value is attached. Such an opinion is not supported by the results of all the longitudinal veins of iron, zinc, and copper mines, hitherto wrought in the United States.

The relics of ancient mining operations consist of excavations found along the line of the veins and of numerous stone hammers scattered around them. Some copper tools have also been found in these excavations. These workings are scattered over the whole mining region and extend even to Isle Royale. The pits are sunk to the depth of twelve or fifteen feet, even into the solid veinstone; they have subsequently been filled with sand and gravel, and on this trees have grown more than one hundred years old. They have therefore this age, beside the time occupied by the slow filling up of the cavities by natural causes. The hammers are so abundant, that at the Minesota mine, I was informed by the agent of the company, fifty cart loads of them might be collected. They are made of a hard variety of trap rock, and resemble in form the hammers or pestles found often in New England. With these, and probably by the aid of fire, to render the rock brittle, the ancient miners penetrated into the solid veinstone, and removed considerable masses of the metal. One of these, found in one of the pits at the Minesota, at the depth of twelve feet, which was abandoned by the old workmen, probably because it was too large for them to remove, after being extracted and cut up, was found to weigh over five tons. This had been perfectly cleaned from the veinstone and all adhering rock, before it was left. It had been partially raised up and was found resting on charred skids of timber, which bore the marks of cutting instruments.

I cannot agree with Dr. Jackson, who has expressed an opinion that all these are the workings of Indians. This race have no use for copper, and there are no traditions of their having sought for it. In the researches of Messrs. Squier and Davis, upon the western mounds, they met with copper chisels, one of which was handed me for chemical examination, to determine, if possible, any indication it might bear of having come from the mines of native metal of Lake Superior. It gave no trace of silver, but was pure, soft copper, like much found in this region. And I cannot but regard it as connecting itself with these obscure, ancient workings. These and the mounds appear to me to be the result of the same race, one more intelligent and skilful than the present race of Indians.

The workings of the English companies, about the year 1670, are known, and are moreover characterized by the remains of iron utensils, which have been found about them.

Should any other relics, as the curiously carved figures of animals in the hardest porphyry, or a single skull of one of the workmen be found, this interesting question would be at once settled.

Dr. Jackson objected to Mr. Hodge's opinion, that the ancient mining operations were the work of the mound builders, for scalping knives have been found among them.

Dr. Moss thought they were the operations of the English, who employed the Indians in the work.

Prof. Rogers spoke with reference to the question whether the veins of copper and iron, enclosed in strata of rock, are true veins, injected after the formation of the strata, or beds formed at the same time as the strata; expressed his decided opinion that they were true veins, and stated facts and arguments to support that opinion.

On the Erratic Phenomena of the White Mountains.
By Prof. Arnold Guyot.

Prof. Guyot remarked of these phenomena as compared with those of the mountains of Europe. He found, in general, in both countries, the valleys near the mountains filled with blocks very similar to those on the tops of the mountains, and extending at great distances from the centres of dispersion. And he advanced the idea that the White Mountains may be a centre of dispersion for a great extent of country around.

Prof. Rogers said this was a most interesting point. We had been disposed to think that the White Mountains were no centre of dispersion. They all agree in pointing to the north and south. If he understood Prof. Guyot, he would think that he considered that there was a centre of dispersion. He believed it was true that the Alps sent out there glaciers in different directions. But having visited the White Mountains and the hills of Vermont, he found there no evidence of any such thing. Instead of the materials having been dispersed in all directions downward, they had been pushed upward, to the height of five thousand feet. This opinion he sustained with a variety of interesting facts, which he illustrated with diagrams on the blackboard.

Prof. Guyot asked whether the rocks beyond the White Mountains were different from those spread around the White Mountains.

Prof. Rogers said that the mountains beyond the White Mountains were a continuation of the Green Mountain range.

Prof. Agassiz said there were two points that should be taken into view. One was, that there were centres of dispersion of erratic materials, in ranges of high mountains. He was satisfied there were indications of this in England, Scotland, Ireland, and Wales. These had been distinctly traced. The result of ten years' examination had confirmed this opinion.

Prof. Rogers said he admitted this with regard to Europe; but the question was, whether the same existed at the White Mountains.

Prof. Agassiz thought that the White Mountains had been under the influence of the general movement from the north; but had since itself become a centre of distribution within narrow limits.

Dr. Jackson remarked that he had traced the drift boulders, which occur at the Wiley House slide, near the notch of the White Mountains, to their parent ledge, north of the White Mountains. While travelling along the road east of the White Mountains — the road which goes from Jackson to Randolph — he observed a slide at the side of the mountains, and on examining the boulders, he found them to be metamorphic argillaceous slates, containing andalusite macles. On ascending the mountain, from which the slide came, he found the slate rocks with the macle crystals in place all along the road; south of this ledge he saw boulders of this rock, and found that they also

constituted a part of the debris that rolled down the mountain side and formed the mass of the slide or land fall at the Wiley House. These boulders are also found as far south as the seacoast at the Boar's Head near Hampton. It appears from the rocks found at the Wiley House land fall, that the boulders were piled up on the side of that hill at a considerable elevation, and formed a steep slope. He referred to his Geological Reports on Maine and New Hampshire for numerous facts illustrating the movements of water and ice floes from the north, and for more curious instances of deflection of the current by the agency of the mountains in its general track. He thought that no evidence had yet been adduced to prove the existence of glaciers radiating from our principal mountain groups, and it was certain that the drift current paid little attention to them, but swept over and around them. There being no instances recorded of the transportation of boulders towards the north, he could not impute their removal to the agency of mountain glaciers, but although the causes of the movement of drifts were still uncertain, he found more reason to believe that currents of water aided by immense ice floes from the north, would account for more of the phenomena than any other agency that had been invoked.

Prof. Hall thought the point not demonstrated, that we have not centres of dispersion, and that the subject deserves further attention. He further remarked, that if the drift materials were all from the north, we should find on the northern slopes of the White Mountains deposits of fragments or boulders of Silurian limestones and sandstones from those rocks, which are found *in situ* farther to the north.

Mr. Redfield believed that the phenomena of the boulders and drift should be attributed to mixed causes, and that the theories which refer these phenomena to the several agencies of glaciers, icebergs, and packed ice, are, in truth, more nearly concurrent than is commonly imagined. It was not simply the action of icebergs as observed by navigators that we were to take in view, but principally annual expansions of a grounded and vastly continuous pack of ice and icebergs, during the long winters; as found even in our own day on the northern shores of our continent. The contemplation of such agencies, through long and successive periods and at different levels of the shores and bottom, together with the casual action of ground-ice and moving icebergs, may go far towards reconciling these different views with a general theory of aqueous and glacial action.

In regard to the theory advocated by his friend Prof. Rogers, he saw not how the fact of a general transport of the drift and boulders from higher towards lower latitudes could be reconciled with its requirements; even if we waive all other objections. For the great centres of elevation and of volcanic action on our globe are so situated as to preclude, apparently, any such uniformity in the direction of transport, by the supposed action of great earthquake-waves, emanating from such centres of disturbance.

Prof. Agassiz said he had seen boulders transported from the Alps to the Jura displaced by glaciers descending from the summits of the Jura; and he could not believe that American geologists were justified to say that there were no similar phenomenon in this country, and that in no instance the northern boulders had been rearranged within the range of the White Mountains, or the mountains of New York.

Prof. Hall said he had seen in the valley near the St. Lawrence, rocks which he believed had come from the South. These boulders and angular fragments were of the same character as the Feldspathic granites of the northern counties of New York, many of them containing Labradorite, and the specimens undistinguishable from those of Essex County.

On the Erratic Phenomena of the Central Alps.
By Prof. A. Guyot.

The diluvian or drift period, the most recent and therefore one of the most interesting in geology, has nevertheless been for a long time one of the most neglected, doubtless on account of the numerous difficulties the study of it presents. Yet the importance of the questions which are connected with it, and the very difficulty of making out the causes, and the mode of transport of the materials which compose this formation, have fixed the attention of scientific men anew upon this epoch in the history of our globe. Various theories of these phenomena have been proposed and warmly debated. After having examined the foundations on which they rest, and being struck with the total insufficiency of our knowledge upon the subject to resolve this difficult problem, Mr. Guyot determined to study carefully the facts of this class upon the classic soil of Switzerland, his native country. He sought to establish by observation and independently of all theory, the bearings and the general laws of

the erratic phenomenon in the central Alps and their vicinity. The results of this study, pursued without cessation during the last eight years, he desires to submit briefly to the Society, adding such considerations as seem to flow immediately from the facts established by this investigation.

The researches of Mr. Guyot have extended over all that part of central Alps comprised between the sources of the Rhine and of the Adda, on the one side, and the point of junction of the Jura and the Alps on the other, in the neighborhood of Chambery in Savoy. They embrace the two slopes, North and South, as is shown by the map of the erratic region exhibited to the Society, but they have had more especially for their object the former, that is, the vast triangular basin of lower Switzerland and Savoy, comprised between the Alps and the Jura.

This portion of the Alps, which comprises Mont Blanc, Monte Rosa, the Bernese and Grison Alps, is the most elevated, and in all respects the most remarkable of the Alpine system. If, on the one hand, the observer encounters great difficulties in traversing this deeply broken region, in penetrating the extent of these desert valleys, and in ascending from glacier to glacier to those lofty summits, to question with the hammer those rocks, covered with eternal snow, on the other, it may be said that few regions seem more favorable for researches of this kind. The vast proportions, and the intensity of the erratic phenomena, the particular disposition of the valleys, and above all, the endless variety of the rocks which compose the different Alpine masses, and which, from one place to another, present always some characteristic difference, which allows us to ascertain their displacement, and to follow their track with certainty; — all these circumstances give to the results obtained a degree of certainty which, perhaps, it would be difficult to attain to elsewhere.

The central portion of the Alpine masses presents, almost everywhere, crystalline rocks, so-called primitive, but probably metamorphic, granite, gneiss, talcose-syenite, serpentine, and other analogous rocks. Farther towards the exterior, the micaceous chloritic and talcose slates prevail: these two groups of rocks predominate in the erratic region. Then come the calcareous and secondary sandstones; finally, along the borders, the tertiary deposits, under the form of sandstone and of conglomerates, forming the high mountains which border the low lands.

The Swiss plain itself is covered with a peculiar tertiary, sandstone, the molasse which fills the basin as far as the foot of the Jura. The limestones which form the Jura are themselves, in a mineralogical point of view, very different from those of the Alps; so that the erratic fragments, coming from the Alps, cannot be confounded with those of the soil on which they lie.

The erratic drift, spread at the foot of the Alps, presents substantially two distinct layers, placed one upon the other. The lower is a deposit composed of pebbles of all sizes, rounded, often stratified, although irregularly, forming strata levelled at the surface, deep and intermingled with beds of sand. It contains no large blocks nor angular fragments: the pebbles are never striated: they are washed and are not accompanied by the muddy slime which characterizes the upper deposit. This terrain, moreover, occupies the bottom of the valleys and the basins, as that of Geneva and Lombardy, and never rises to the heights which border them. Fossils, elephants' teeth, lignites, are found there, though rarely. This deposit has received from Swiss geologists the name of the Ancient Alluvium, (*Alluvion ancienne.*)

The upper deposit is the deposit of blocks, the erratic group properly so called. It is composed of pebbles, angular fragments, and especially of blocks of every size, angular or rounded. All these fragments are thrown, as it were, pell-mell, without regular stratification, without being sorted according to their size; so that often in a section of more than one hundred feet in thickness, we see blocks of several feet in diameter, scattered indifferently through the whole height of the layer, in the midst of the minutest materials. Nevertheless, the largest blocks, and the angular blocks are more on the surface, or at a small distance under the soil. All these fragments are accompanied by a miry slime, or by a characteristic earthy sand, and the pebbles are deeply scratched. Finally this drift covers not only the low places, but it shows a marked preference for the heights, where the large blocks are accumulated in greater numbers than in the low bottoms. Moreover, it rises several thousand feet above the plain, and forms the upper limit of the erratic terrain. Mr. Guyot has occupied himself especially with this upper deposit, and with the mode of dispersion of the erratic blocks. The results of these researches may be summed up as follows:

1. The chain of the Alps is the central mass, whence the erratic fragments spread over the valleys and the plains which sur-

round it, have descended, branching out in all directions. The extreme limits of the space where these boulders are met with, are usually clearly traced, and the region of the Alpine erratic boulders, although touching at some points on those of the Vosges and the Black Forest, is not confounded with them. There is neither intermingling nor insensible transition from one to the other.

2. The space comprised between the Alps and the Jura presents seven great groups of rocks or erratic basins, differing from each other, each group corresponding to one of the great Alpine valleys, which open upon the plain. On the Southern slope we distinguish in the limits indicated, four great erratic basins, in which the particular grouping of the valleys, and the bifurcation at their extremity, introduce some modifications.

3. The limits of the erratic basins touch each other for great distances without their respective rocks intermingling in any sensible degree.

4. In the interior of each basin also, the different kinds of rocks taken separately, are not absolutely intermingled; they tend to form linear series, which keep the same relative situation in the plain that the rocks occupied in the mountains; so that the rocks of the lower borders of the valley, keep to the respective borders of the basin, while the others are situated further towards the interior of the basin, in proportion as they come from points nearer the origin of the valley.

5. The volume of the blocks and of the erratic fragments does not go on decreasing in proportion as they are found farther from the Alps. The largest blocks, on the contrary, are generally situated at the extremity of the basins, at points the most remote from their place of origin, and often form, as in the Jura, the extreme limit of the Alpine debris, which stop suddenly with them.

6. There exists, in the Alps themselves, as in the Jura, a superior limit, above which the erratic blocks and the pebbles are not found, and which indicates the existence of an ancient surface level, descending regularly from the Alps to the Jura. This limit is 9000 feet, at the centre of the Alps at the point of departure of the erratic fragments; 4000 or thereabout at the outlet of the valleys; 3700 feet on the Jura, preserving almost everywhere, a relative height of about 2500 feet above the neighboring bottom. Yet on the sides of the Jura, this limit, from its maximum point lowers itself gradually towards the West, and descends to the East almost to the level of the plain.

7. The presence of the blocks is intimately connected with that of the polished, rounded, furrowed, and striated rocks. The limit of both is the same. The polished and striated rocks are found only in the interior of the basins, and not beyond. In the interior of the valleys of the Alps, the erratic blocks commence with the rounded rocks, above which we meet only with rocks which have fallen down from the broken peaks at the foot of which they are found.

Such are the bearings of this great phenomenon, and the principal laws which regulate the distribution of the erratic blocks. These general facts once established, the field of the hypotheses which can account for them, is singularly narrowed.

The hypothesis of great currents of water, of a great breaking up, which at the first view seems the most natural, is no longer admissible after a thorough examination of the facts. The upper limit of the erratic deposit compels us to admit that this current must have had its point of departure, at the bottom of the valleys, in their narrowest part; and yet its depth was 2000 or 2500 in the plain as well as in these valleys. In these circumstances, whence could a mass of water sufficient to form such a current come? How could this current, spreading out in the plain, leaving the narrow valley that served as its bed, nevertheless preserve the same level and the same depth, although it had become twenty or thirty times as broad? How could it transport blocks of sixty feet in length, suspended even near the surface, over the mountains and across the lakes? How, notwithstanding its violence, could it transport these intact and still wholly angular, in regular lines on the sides of the Jura? How, finally, could it suddenly stop in the middle of its course, without any obstacle opposing its spread, and at the moment when, to judge by the elevated situation of the blocks it deposits, it had still, as beyond Geneva, a depth of more than 1500 feet? It is obvious that in this way we proceed from improbability to improbability.

It has been attempted to meet some of the objections by supposing this current animated by a velocity which the imagination refuses to conceive, and which, hurling the blocks against the Jura, would have reduced them to powder instead of depositing them gently in the places where they are preserved. Others have held that this current had the consistency of a thick mud, on which the blocks were supported. But how could this immense bed of mud and of Alpine materials have disappeared without leaving a trace, without even marking the path by which it must have flowed? To lighten the

troublesome weight of these blocks, others have had recourse to the ices of the glaciers of the tertiary epoch, which melted, as they say, by the subterranean fires during the last upheaval of the Alps, descended in large fragments, laden with blocks in the midst of the muddy currents, which were the consequence of this sudden melting. But who does not see that while weakening one of the preceding objections, this hypothesis raises others quite as grave, without solving any better the problem of the regular arrangement of the erratic fragments. The idea of one or more immense waves, which, in consequence of some violent shock of the earth must have swept the continent from north to south, and breaking against the Alps must have carried in their retreat the erratic boulders, has the same objections, and adds one difficulty more: for in that case we do not understand the complete separation of the domain of the Alpine erratics from that of the neighboring systems, nor why the rocks of the north, particularly those of the Vosges and the Black Forest, should not have been swept away in this catastrophe, and mingled with those of the Alps. Finally, the theory of the transportation of the blocks by floating ice, a theory which has its truth elsewhere, here fails entirely to account for the facts. Applied to Switzerland, it supposes an interior sea, of which we discover no trace, and the existence of which cannot be admitted, except by supposing, against all probability, great changes in the present configuration of the ground. It would require, besides, the upper limit of the blocks, which should mark the shore of this sea, to form a level line, while it is everywhere inclined, whether it be of the Alps or the Jura, or upon the sides of the Jura itself.

But it is especially one of the laws announced above, in the explanation of which all the preceding theories fail equally, — namely, that of the arrangement of the different kinds of rocks in parallel zones, in the interior of each basin. This arrangement is in fact absolutely incompatible with the tumultuous action of any torrent whatever, especially if we take into view the violent changes of direction, and the eddies which they experience in the valleys, (as in that of the Rhone at Martigny,) the inevitable effect of which must be to mingle entirely all the materials which they carry along. It is no less so with floating ices, which disperse and break up in all directions, without order, according to the caprice of the winds.

Among the agents in operation before our eyes, there is only one which produces an assemblage of phenomena like those which be-

long to the Alpine erratic drift; and that is the glaciers. The laws stated above are no other than the laws of the morains, or the blocks transported on the surface and along the edges of the present glaciers. Mr. Guyot exhibits to the Society a diagram, showing the distribution of the blocks of the glaciers of the Aar, and comparing this map with the erratic map of the whole of Switzerland, he demonstrates the complete identity of the two phenomena. Not only, he adds, is there identity, but continuity from one phenomenon to the other. The blocks, angular and arranged in lines, the striated pebbles, the rocks, polished, rounded and grooved, commence with the glaciers and only end with the erratic deposit. And this deposit in the Swiss plain presents the same aspect as would strike the eye of the observer, if all this basin were filled with vast glaciers issuing from the Alps, and covered with their morains. Only the ice has disappeared by melting, leaving upon the soil its covering of blocks, without changing anything in their arrangement.

Thus are explained the separation of the basins, the grouping of the species of rocks in parallel and linear zones, their distribution in special localities, their respective situation, always conformed to the position of the places whence they have issued. By means of the law of central or medial morains we account for the remarkable fact that the blocks which start from the most distant valleys, and from the highest summits, are also those which, in spite of their volume, depart the farthest from their primitive position. The preservation of the blocks, their angular forms, their passage across the lakes, their sudden cessation, and the constancy of the upper limit, all these phenomena, in a word, for which no hypothesis accounts in a manner at all satisfactory, are no longer in our eyes an impenetrable mystery.

In presence of such facts, it is sound logic to conclude that the transport of the erratic blocks of the central Alps has been brought about by means of the great diluvian glaciers. This conclusion, it is well to remark, does not rest upon any hypothesis whatever, nor upon a scaffolding constructed at pleasure by the imagination, but upon the identity and continuity of two groups of facts, carefully observed and henceforth explained,—the mechanism of the present glaciers, and the mode of dispersion of the erratic drift.

If the existence of a glacial epoch, or even the possibility of the great diluvian glaciers has been called in question, it is in the opinion of Mr. Guyot, because it has been ill understood. It has been too

much forgotten that the condition of the formation of great glaciers is not an intense cold, but a great abundance of snow, that is, of humidity. It is not then to a sudden cooling of the globe by an unknown cause, — which would be little conformable to the analogy of the physical laws, — that Mr. Guyot attributes the glacial epoch, but solely to a temporary perturbation in the hygrometrical state of the atmosphere of the countries in which these erratic phenomena have been observed. Now, without resorting to any hypothesis, but basing our conclusions on known and established facts, we may affirm that an atmospheric disturbance of this kind has taken place in the regions of the Alps, at the end of the tertiary epoch, at the beginning of the epoch which succeeded, or at the diluvian epoch. The hollowing out, in the tertiary soil, of broad and deep valleys, in which the present rivers wind at ease like brooks, and the deposition of thick beds of the ancient alluvium or of the lower drift, are alone sufficient to demonstrate the power of currents of water, and the extraordinary abundance of the rains at the epoch of that crisis. All these waters necessarily falling in snow during the winter of the climate of the temperate regions, covered the whole ground with a layer which the heat of summer could not cause to disappear entirely. The accumulation continued as long as this abnormal state of the atmosphere lasted, and the result was the existence of vast glaciers, formed on the spot, covering the ground with a thick layer of ice, and moving on, as usual, with their blocks, following the natural slope of the drift on which they rested. The formation of glaciers of 2000 or 3000 feet in thickness, under the influence of such atmospherical circumstances, will not seem improbable, if we remember that a direct measurement of Prof. Agassiz gives to the present glacier of the Aar, in its upper portion, a thickness of at least 850 feet, which may be one thousand, if we judge it by the structure of the valley, and that, on the other side, the wet summers of 1816 and 1817 were sufficient to cause in the glacier of la Brenva, on the slopes of Mont-Blanc, an increase of at least 200 feet in thickness and over one mile in length. The presence of these masses of ice during the whole year, and over vast areas, gradually lowered by several degrees the mean temperature of the regions where they were found, and brought on the cold and moist climate which the fauna of that epoch indicates. At a later period the equilibrium of the atmospheric waters having been restored, the summer heat melted each year a quantity of the ice greater than that which the winter had furnished, the diluvial glaciers

gradually lessened, the temperature rose little by little, and at its entire disappearance, returned to its present state.

The phases of the phenomena in the region of the Alps, indicated directly by the facts, are then the following: —

1. Rains of extraordinary abundance and long continued, give birth to powerful currents of water which hollow out the valleys in the tertiary soil, and deposit in the bottoms the ancient alluvium, with fallen pebbles and without blocks, and bury there the last fragments of the organized beings of the closing epoch. But these torrents have nothing in common with the monstrous currents of 2500 feet in depth, which, according to some hypotheses, would have deposited the erratic blocks on the heights. This is the epoch of the deposition of the lower drift, or of the deposit with pebbles.

2. The atmospheric waters falling in snow are converted into glaciers, the presence of which makes the climate considerably colder, and which carry along the Alpine blocks in their course, and deposit them even at great heights. This is the epoch of the establishment of the upper and extreme limits of the erratic terrain, properly so called, or the upper drift, or drift with blocks.

3. The equilibrium is restored in the atmospheric waters, which return to their normal quantity; the glaciers less copiously fed, gradually melt, disappear and leave the blocks which they carried on their surface, and the triturated fragments on which they moved, lying upon the ground which they have occupied. The temperature rises in proportion as the cause of its depression ceases to exist. This is the epoch of the principal deposition of the blocks and of the formation of the accumulations of glacial alluviums.

Thus, according to Mr. Guyot, the glacial epoch is the consequence of a disturbance, not of the temperature, but of the hygrometrical state of the atmosphere, attested by the geological facts, the causes of which he reserves to himself to inquire into on another occasion.

The lowering of the temperature during this epoch is the *consequence* of the presence and not the *cause* of the diluvial glaciers.

What confirms the justness of these views is, that the erratic phenomena, properly so called, are confined to the polar and temperate regions, and cease to be observed where the climate does not permit the atmospheric waters to fall in snow.

Such seem to be the principal phases of the diluvian epoch in the region of the Alps. But it is readily conceived that with the same

general features they may present themselves in a very different manner in other countries and under wholly different physical circumstances, as in Scandinavia and North America. It is from the carefully conducted study of all the memorials that remain of this important period in the history of our globe, and not from hypotheses independent of rigorous observation, that we must henceforth demand the solution of these interesting questions.

PROF. AGASSIZ explained the manner in which the Swiss geologists had come to their conclusions. They had divided the work of investigating the phenomena of the Alps, and had thus been able to study every portion thoroughly; and the map by which Prof. Guyot had explained his views, was thus produced. Prof. Guyot had undertaken to investigate the Alps, beyond the limits where the glaciers are now seen. He himself had devoted his attention more particularly to the glaciers. For ten years had Switzerland been studied in this systematic manner. By taking maps of the distribution of boulders in those places where the glaciers are now found, and where the phenomena be seen to result from glacial action, and maps of their distribution where the glaciers are not now extended, and placing one upon the other, as geometers show the identity of forms, the identity of these phenomena is proved. What matter was it whether Mr. So-and-So or Mr. So-and-So was prepared to admit the theory, when the facts were before their eyes, showing that it had been so?

During the ten years' observation of the Alps, 3000 points had been barometrically observed; and an immense amount of detail had been ascertained, as yet unpublished, which he believed would change the views of those who were now so unwilling to admit the glacial theory, at least so far as regarded the Alps and the Jura.

REV. P. LESLIE. The world was making a very low, though it might be a very slow bow to the Swiss geologists, in recognizing the minuteness, as well as the vast extent of their investigations. All that was now to be done was to ascertain the truth of the geological conclusions to which Swiss geologists arrived in the process of their investigations. In speaking of the ancient diluvial deposits beneath the erratic boulders, and in some other cases, Prof. Guyot had been obliged to assume points which might be questioned, and which the investigation of the Alps alone, perhaps, would not determine. He would inquire in what light Prof. Guyot regarded these diluvial strata.

Prof. GUYOT replied, that he had omitted this, simply because it

did not come within his immediate view; and proceeded to explain the subject.

Prof. ROGERS said, that if a true test could be determined between glacial and diluvial action, it would be easy to get at the *vera causa*. He contended that the lines indicating the dispersion of the erratics of the Alps in the Plain of Switzerland, were those of *progressive* deposition, and not of retrocessive deposition, and showed the difference of the action of the two, observing that the lines intimated water and not ice.

Prof. AGASSIZ considered the continuation of the phenomena a true test. There was no interruption between the end of the present glacier and the phenomena below; and to call the one water action, while we know the other to be the action of ice, would be absurd.

Prof. ROGERS would admit that the glaciers had extended below their present limits, but believed that he could discern the action of water also. There had been the action of both.

Prof. AGASSIZ said that no theory which introduced the agency of water, or any other agency than that of ice, within the limits of Switzerland, could be correct.

Mr. BOND inquired whether the largest boulders were found nearest the gorge, or not.

Prof. GUYOT stated that such was not the case.

Prof. HALL agreed with Profs. Agassiz and Guyot, in the conclusion that ice had brought about these facts. In this country there had been a tendency to oppose the glacial theory, because it had been applied for the explanation of diluvial phenomena. We ought to be ashamed in this country, he said, to speak with confidence, after so little research, when there had been so much abroad. He considered the evidence conclusive that the phenomena of the Alps could only have arisen from the action of ice.

ON THE TREND OF ISLANDS AND AXIS OF SUBSIDENCE IN THE PACIFIC.

BY JAMES D. DANA.

THE Pacific Ocean to latitude 66° S. covers more than 62,000,000 of square miles, and exceeds by 10,000,000 of square miles the area of all the continents and known lands of the globe. Its islands are confined with few exceptions to the latitudes within the tropical circle: New Zealand and a few associates on the south, and some

small points northwest of the Hawaiian Group, are almost alone in exceeding these limits.

The ranges of islands throughout the Ocean conform to a system in their courses, which is remarkable for uniformity and extent. The eye at once discovers a prevailing trend from the northwestward to the southeastward, and a general parallelism in the directions of distant groups. The Hawaiian, Marquesan, Paumotu, Tahitian, and Navigator islands, New Hebrides, and New Caledonia, show well this parallelism of trend. The *Hawaiian* Range continues westward to 175° East longitude, at an angle of N. 64° W.; the Marquesas Islands are nearly parallel, trending N. 60° W.; the Paumotus have the same course, N. 60° W.; the Tahitian Range trends N. 62° W.; the Hervey Group N. 66° W.; the Navigators N. 68° W.; New Hebrides N. 40° W.; New Caledonia N. 44° W.; and the same is the course of the northeast coast of New Holland, and the coast of North America opposite. Thus across this wide ocean, through a breadth of 6000 miles, there is this striking correspondence.

We observe further, that the Society and Hervey Islands, together with the Navigators, are in a great central range of the Pacific, which gradually curves to the northwestward. In the Fakaafo Group, just north of the Navigators, the trend is N. 58° W.; in Ellice's, N. 56° W.; in the Kingsmills, N. 42° W. to N. 25° W.; in the Marshall Islands, the termination of the great chain, N. 37° W. to N. 30° W. Thus this great Pacific chain, as it stretches west gradually curves northward, (or is convex southward) and makes a total course of 3800 miles.

The Hawaiian chain has a very slight curve, which is convex northward. Its whole length is about 2000 miles. The Marquesan chain, if we include the Fanning line, is a straight range 1500 miles long. The Paumotus stretch on for 1500 miles.

New Hebrides and New Caledonia pertain to a range that curves westward to New Guinea and Java, and thence northwestward by Sumatra. From New Hebrides, whose trend is N. 40° W., we observe next the Vanikoro or Santa Cruz group, trending N. 44° W.; Solomon Islands N. 50° W.; New Ireland N. 65° W.; Admiralty Islands N. 85° W., thus gradually changing to east andwest.

There are also *transverse* courses in the Ocean. New Zealand lies in a transverse direction, trending N. 30° E., and this line is continued in the Kermadec Islands north, and also farther north in the Tonga group, trending N. 22° E. The line is nearly at right angles

with the Navigators, across whose position the chain points; it is the great transverse range of the Pacific.

The idea of mountain chains in an ocean is no hypothesis; for islands are but the culminant points of such chains; and could we lay bare the Pacific we should see some of the most extensive ranges in the world, all preserving a systematic regularity which we distinguish with difficulty in the mountains of the continents.

There are many other considerations of deep interest which must be passed by for the present. We may next consider *the axis of Subsidence for the Pacific Ocean*, on which point the above facts have an important bearing. Has there been a general axis of Subsidence for the Ocean? What has been its position and what its epoch?

At a former meeting of this Association, I presented some remarks on the Area of Pacific Subsidence indicated by Coral Islands. It was stated that if a line be drawn from the Admiralty Islands, (north of New Guinea) by the Navigator group and the Society range to the Gambier Islands, this line would be a boundary between the many coral islands on the north and the high islands on the south. The Carolines north of this line, are coral islands excepting three; the Paumotus are of coral; and so also the many scattered islands between these great Archipelagoes. Accepting of Darwin's theory that coral islands have been formed by subsidence — that each originally was a fringing reef to a high island, and as the island subsided the reef still continuing to grow, became finally (as the high island disappeared in the waves,) a coral atoll, (and I have had abundant opportunity to satisfy myself of the truth of the theory) — we have direct proof that north of the boundary line laid down, a general subsidence has taken place. The islands near the line are much larger and more numerous than over the seas farther north towards the equator; and just north of the equator there is a region almost destitute of even small points of land. Hence the subsidence, which was comparatively small south of the boundary line, was greater and greater northward; and so extensive just north of the equator as to have caused a disappearance, not only of the high islands, but also of the coral reefs that may have bordered them. About 250 coral islands remain in the Ocean as monuments over buried lands; and if we may assume that some of these lands were as elevated even as the mean height of existing islands in the Pacific, 6000 or even 10,000 feet can hardly be an over-estimate for the amount of subsidence that has taken place.

Beyond the equator, latitude 20° to 25° N., lies the Hawaiian group, which, like the high islands south of the boundary line, show little evidence of subsidence. The great area, then, which has undergone the supposed change in the coral island epoch is mostly included between the Hawaiian Islands on the north, and the Society and Navigators on the south. A line drawn from Pitcairn's Island, and crossing the equator at 162° W. longitude, continued to Middle Japan, latitude 36° N., passes through the region shown by the facts stated to be that of greatest subsidence. The western part of the long Hawaiian range, which part is nearest this axial line, consists mostly of coral atolls, and therefore a greater subsidence has there taken place than farther eastward in the range, a fact supporting our general deduction. From the evidence afforded by reefs, we learn approximately the amount of subsidence at the different groups bordering the great area. At the Gambier group it is about 1150 feet; at the Society Islands, 250; at Samoa, 100; at Kauai, of the Hawaiian group, not over 100, and probably less. On the side of the area opposite to the Gambier group, lies the Marquesas range, a collection of high islands, indicating much subsidence by their features, but certainly evincing less than over the Paumotus southwest. The Feejees afford evidence of considerable subsidence, but less on the southern than northeastern side. We are therefore safe in laying down the line proposed as the axis of subsidence for the coral island epoch.

After arriving at these conclusions, the subject of *the trend of islands* was under consideration, and it confirmed the above conclusion in a most perfect manner, proving not only that this axis was the axis of subsidence for the coral island epoch, but for a period long previous. It will be observed that the great Pacific chain, in its long course across the ocean, has a convexity to the *southward;* the Marquesan range, including the Fanning group, has a straight course; the Hawaiian range has a slight convexity *northward*. Here, then, we have great curving lines stretching through the ocean, and central elliptical area for the Pacific indicated. The straight course of the Marquesan range implies its nearness to the axis of the area, and the opposite curvatures of the Hawaiian and great central chain of the ocean, their positions on opposite sides of the area. In view of the facts, we deduce for the axial line thus marked out, a position nearly identical with that derived from the distribution of coral islands. The two results were independent deductions in my own mind, and seemed to give mutual confirmation.

This *axial line*, it will be observed, *lies in the course of the longest diameter of the Pacific*, stretching southeast, into the Southern Ocean beyond Cape Horn. Moreover *it is parallel with the mean trend of the Pacific islands*. These are two considerations of great weight.

Can we doubt, then, that the ocean has been long in the process of subsidence; — that its features were fixed in the very nature of the earth's structure, and were begun in the early history of our globe? Can we hesitate to admit that the vast Pacific Ocean is one in origin and history, and has had approximately one and the same axis through its long course of progress?

In the above remarks I have not alluded to existing subsidence or changes of level. It is remarkable that evidence of subsidence in progress is apparent in the Pescadores and some other northern Carolines — the islands nearest the axial line of greatest subsidence, and where, if this general sinking was still in progress, we should expect to find proofs of it. These coral islands are nearly bare of vegetation, a condition which indicates that subsidence is still going on; for when subsidence ceases, the coral atoll commences to become covered with vegetation.

To appreciate the full bearing of the facts stated, it should be kept in mind that the Pacific is no area of small limit like the region of the Alps, or the Urals; neither has it the contracted territory of Europe, or Asia. It actually embraces a larger surface than all the islands and continents together of the globe; and in its narrowest direction it includes one-fourth the whole circuit of the earth. A law which has so wide an application, must be of fundamental importance.

ON THE GEOLOGY OF CANADA. BY T. S. HUNT.

MR. T. S. HUNT, of the Geological Commission of Canada, made an oral communication upon the results of the geological exploration of that country, and showed by the aid of a map, the general distribution of the formations and their relation to the rocks of New England. The following is a summary of his remarks: —

In presenting the report made to the Provincial Government, by W. E. LOGAN, Esq., embracing the results of the survey of 1847–8, I beg leave to offer a brief sketch of the results which have been developed by himself and his assistants. The feature which first claims our attention in looking at the geological structure of this country, is a

formation of syentic gneiss, often passing into mica schist, and interstratified with crystalline limestone which forms a ridge of high land extending from the coast of Labrador along the north side of the St. Lawrence, at a distance of from twelve to twenty miles from the shore, until it crosses the Ottawa, near Bytown, and thence is traced across Lake Simcoe to the shores of Lake Huron, where its northern limit is observed near the mouth of the French River, while it again appears at the south-eastern extremity upon Matchedash Bay. Resting upon this is a series of rocks forming the whole north coast of the lake, and numerous small islands. It is made up of sandstones, often coarse-grained, and sometimes becoming conglomerate from the presence of red jasper pebbles. These beds are associated with slates, and one or more bands of limestone. The slates are greenish, and highly chloritic, often containing epidote; sometimes they assume the character of conglomerates, from the presence of pebbles of syenite. The formation is much cut by greenstone dikes, and exhibits very frequently interstratified beds of greenstone, often of great thickness. Both these and the sedimentary beds contain metalliferous quartz veins, of which the copper mines of this region are examples. Resting unconformably upon the tilted edges of this formation, and in other places directly upon the southern limit of the syenitic gneiss, appear the silurian rocks, identical with those which are found in New York, and cover the peninsula between Lake Huron and Lake Ontario. Beginning with the rock designated in the New York nomenclature as the Potsdam sandstone, we have upon the Manitoulin Islands and the coast between Matchedash Bay and Sarnia, a complete exposure of those formations known as the Trenton limestone, Utica slates, Loraine Shales, Medina sandstones, and the Niagara limestones, with the rocks of the Clinton group. All of these are well characterized by their respective fossils, and are spread out quite undisturbed at a very gentle dip of about thirty-five feet in a mile. The thickness of these rocks, as exhibited in a section across the Grand Manitoulin and La Cloche Islands, was found to be from the base of the Potsdam sandstone to the top of the Niagara limestone, 1,273 feet.

Passing to the East, we find that the syenitic rocks have divided near where they cross the Ottawa, and taking a southward course, are spread over a considerable extent of country between the Ottawa and the St. Lawrence. Crossing this river below Kingston, they constitute the greater part of the Thousand Isles, and are extensively developed in the northern counties of New York.

The country thus bounded on the west and north, consists of a broad valley of twelve to twenty miles on the north, and thirty to forty miles on the south side of the St. Lawrence, which at its south-western extreme, includes the valley of the Richelieu and the northern part of Lake Champlain. On the south-eastern side of this is a mountain belt of from twenty-five to thirty miles in width; this is the prolongation of the Green Mountains of Vermont, which further north constitute the Shickshock and Notre-Dame mountains of Gaspé. This mountain range, coincident with the course of the river, is bounded at its south-eastern base by a valley of gently undulating land from twenty to thirty miles in width, which may be traced from the upper part of the Connecticut river to the upper portion of St. Francis; thence by the eastern branch of the Chaudière to the Rivière de la Famine, a tributary of the Chaudière, the valley is continued in the course of the St. John's, until further on it falls into the valley of the Risstigouche, and is thence traced quite into the Baie de Chaleurs. The general strike of the rocks is coincident to the direction of the St. Lawrence and the mountain range, and the same geological formations appear to be continuous without.

If a line be drawn from St. Scholastique, upon the north shore of the Ottawa, passing forty miles S. E. to Montreal, and thence to Canaan, on the Connecticut river, in the north of Vermont, we shall have a section nearly at right angles to the general course of the formations. Commencing at the north-west, the first rock which presents itself resting upon and skirting the bases of the hills of syenitic gneiss and crystalline limestone, is a fine sandstone which is seen on both shores of the Ottawa at its mouth, and constituting there a considerable island, is thence traced south into the county of Beauharnois, where it spreads out a considerable width, and passing into the State of New York, divides against the syenitic formation. Sweeping around its base, one portion passes up the valley of the St. Lawrence, and the other is developed in that of Lake Champlain, where it is recognized as the Potsdam sandstone. To the north-east it probably skirts the base of the syenitic rocks, and has indeed been observed at the falls of the St. Maurice, but owing to the great depth of tertiary deposit which fills the valley, the opportunities of examining it are but few. The next rock upon the line of section is a limestone, very silicious at the base, but pure and thick-bedded in the middle, gradually becoming bituminous and shaly toward the top. This formation, exposed at a very moderate dip, constitutes the greater portion

of the island of Montreal, and crossing to the north side of the river, is lost beneath the tertiary sands and clays. To the south it sweeps around the extremity of a trough, until it reaches St. John, where either turning over an anticlinal or affected by a dislocation, it turns up the west side of the Richelieu and runs into New York. This formation is shown by its fossils to be referable in its lower part to the calciferous sandstone, while the upper beds are the Trenton limestone. It contains interstratified greenstone trap, sometimes amygdaloidal, which constitutes the mountain of Montreal.

Resting upon these calcareous rocks is a set of black shales, which appear on both shores of the river before Montreal, and constitute some islands in its bed. To the south these shales, which are the Utica slates, follow the course of the limestone, keeping the east shore of the Richelieu, and spreading out to a considerable breadth, constitute the region of country between the mouth of the Lake Champlain and Missisquoi Bay. To these succeed a series of shales, bluish and grayish, arenaceous, and more or less calcareous, which are evidently from their fossils the Loraine shales. These are seen upon the Richelieu at Chambly, upon the Yamaska, near St. Hyacinthe, and in several other points along the line of strike. They present a considerable breadth, and are not improbably kept at the surface by some little undulations. Succeeding these, after two or three miles, covered by tertiary sands, appears a repetition of the Trenton limestones, which have been traced from Philipsburg, upon the line of Vermont, through the seigniory of St. Hyacinthe, to Deschaillons, where they cross the St. Lawrence, and are again exposed upon the northern shore. These are followed by a repetition of the Utica slates and Loraine shales, which flank the limestones upon the St. Lawrence, and are exposed at various points along the strike. Upon the Barbue River in the seigniory of St. Hyacinthe, occurs what appears to be a small trough of higher rocks, consisting of heavy greenish sandstones, interstratified with red and chocolate colored slates, sometimes mixed with green bands. These red slates are highly ferruginous, and sometimes contain traces of oxide of titanium. Near the line of Vermont, appears, succeeding the Trenton limestone, the extremity of a similar trough of slates and sandstones, more or less calcareous, which is prolonged into Vermont. In Yamaska mountain a mass of trap lies in the line of the St. Hyacinthe sandstones and red slates, and brings up on its flanks similar sandstones and bluish and greenish slates, with a crystalline yellow-weathering limestone, which after-

wards becomes an important characteristic. The sandstones near the trap contain mica and plumbago.

These rocks however are not seen upon the line of section, but in their strike occur the bluish and grayish calcareous and arenaceous shales, which are followed by light greenish and ash-gray slates, interstratified with gray sandstones. Following these appear the red slates with green bands and their accompanying sandstones, which are sometimes finely conglomerate and more or less calcareous, often containing mica and graphite. These are associated with bands of a greenish chloritic limestone, holding small portions of oxide of chromium in some form, and near the base, with one or two beds of grayish limestones. South of this section line, the strata on each side of this deposit converge, but northwardly the breadth gradually increases, and seems to show that these rocks form a trough more or less disturbed by undulations. Following the western side of the trough, the slates with their accompanying sandstones, cross the St. Francis, are seen at St. Nicholas on the St. Lawrence, and in the rear of Pt. Levi near Quebec. On the eastern side, the slates are followed to Roxton, where, affected by an undulation, they sweep round towards Shefford Mountain, and thence are traced to Inverness on the Becancour, accompanied by the beds of limestone, already mentioned as associated with them at the base. Beyond these, on the line of section, is a set of gray and black clay slates, with thin bedded sandstones and limestones, which although presenting no fossils, appear in their general characters identical with those on the other side of the trough and with the fossiliferous beds of the Richelieu and Yamaska. These rocks appear to run upon an anticlinal to Shefford, where an undulation has been described as carrying the sandstones to the east; thence upon another anticlinal across the Nicolet, where the dark slates and limestones are traced around into a narrow anticlinal valley which runs parallel with the other anticlinals, and is continued to the province line in the township of Sutton.

This double trough contains two great masses of trap, which constitute Browne and Shefford mountains, and appear to have disturbed and altered the rocks to a considerable extent. South of these intrusive rocks we have first upon the section greenish and gray clay slates, followed by a belt of silicious and calcareous rocks, which vary from a somewhat arenaceous limestone to a feebly calcareous sandstone. These are seen in some places divided into three bands by the

intervention of clay slate, probably by undulations, which produce repetitions.

The limestone is dolomitic, and is cut in all directions by great numbers of quartz veins, and sometimes contains garnets, and is associated with iron and copper pyrites. At the distance of about a mile is another band of limestone precisely similar, and accompanied, like it, with slates and quartzose beds, which seem to be altered sandstones, and make the first high lands of the mountain district. This ridge, with its two bands of dolomite, appears to be synclinal, and it is traced about ten miles from the province line, where it dies out. Here another hill, about half a mile to the S. E., apparently an anticlinal, takes up the same measures.

To these succeed a series of more or less quartzose chloritic rocks, with an imperfect slaty cleavage. They seem to be altered sandstones, which upon their western border, where the alteration has been less profound, still present their original structure. Following these, appears a band of limestone resembling the last, and often divided into two or three belts by green chloritic or gray talcose slates, interstratified with beds of an impure specular iron ore, more or less mixed with chlorite and often titaniferous. The limestones sometimes contain green and purplish talc and occasional crystals of chromic iron; they are marked by the same quartzose veins as before. About two miles farther on, a precisely similar belt occurs, and the interval is filled with talcose, chloritic, and epidotic slates, associated with bands of magnetic and specular iron. The epidote forms little nodules, and is often associated with quartz; rutile with specular iron is sometimes found crystallized in quartz veins. From this, extending to the Sutton valley, which is the supposed prolongation of the anticlinal, is about a mile of strong quartzose rocks, slightly chloritic.

Another section presented upon the St. Francis, which cuts the rocks nearly at right angles, shows the dark colored slates and limestones supporting greenish silicious slates, which are followed by a belt of brown weathering dolomite, interstratified and accompanied with purple sandstones and red slates, to which succeeds at a distance of about a mile, another belt of limestone, with quartzose bands. The intermediate rocks are sandstones and conglomerates, often almost pure quartz; south-east of the limestone are seen two or three miles of chloritic rocks, with nodules of epidote and quartz, followed by a band of dolomitic limestone, with veins of magnetic and specular iron ore, and associated with talcose slate. To this succeeds an

extent of heavy quartz rock, slightly talcose, and another band of dolomite interstratified with talcose slate, which is followed by the same fine greenish silicious slates as were observed on the western side. Beyond these are found the dark slates with their thin limestone beds, which, as has been already remarked, have been traced around to the opposite anticlinal. The fact that the dolomites and sandstones within, are traced around in the same manner, and the similarity in their lithological characters, shows that they are on opposite sides of a synclinal.

On the line of section about a mile beyond where the Sutton dolomites would cross, occurs another belt of dolomite associated with soapstone and green chromiferous talc. In its strike we find in one place a band of soapstone and filled with bitter spar, and passing on the north-west into a dolomite of the usual character, while on the south-east is a narrow band of green serpentine. Another dolomitic band occurs a little farther, associated with green talc, serpentine, and soapstone. It has been followed for a considerable distance, and in one place consists of soapstone with patches of dolomite which in a distance of about three hundred yards on the strike, passes into a band of dark green serpentine with soapstone. At other places in the strike, the soapstone is associated with chromic iron, and in one place a bed of magnesite with chromiferous talc. These appear to constitute a trough, and the interval is filled with coarse quartzose chloritic slates, occasionally epidotic, with imbedded crystals of magnetic and specular iron; mica and felspar are not unfrequently met with.

Following this, the rocks for the next five miles are coarse chloritic micaceous schists, often felspathic, passing into gneiss, and in some places, very quartzose. About three miles on the line of section, is a band containing talc and calcareous spar, the latter making a considerable portion of the rock, which is stained green with oxyd of chrome. East of this, the rock is more felspathic and contains small crystals of tourmaline. These measures as they go south, expand into a wide mountain tract, the summit of which, Sutton mountain, is more than four thousand feet above the St. Lawrence. A valley in the line of the chromiferous calcareous rock, divides the mass into two ridges, one of which dies down very soon, while the other crosses the line of section and is lost a few miles farther north; this region still requires further examination, to determine accurately the relations of the western portion.

On the eastern side of this range occurs a belt of soapstone and

serpentine, which has been traced at intervals, a distance of twenty miles along the west borders of the Missisqui. On the west it is bounded by a quartzose chloritic band, associated with a translucent silicious rock of a corneous lustre and fracture. In the strike of the serpentine further north, domolite is found. On the east side of the river, at the distance of a mile and a half from the former, another serpentine band occurs; the interval is filled with slates and gray quartz rock, with some beds of chloritic and epidotic rocks and a curious jaspery quartz rock containing epidote. This band of serpentine has been traced one hundred and thirty-five miles from the province line, across the Chaudière, to the township of Cranbourne. In some parts, it seems to pass into, or is associated with, a diallage rock, and in others to be a mixture of quartz and serpentine. Like the western band, it is accompanied with soapstone and contains veins and disseminated grains of chromic iron.

Beyond this occur clay slates with beds of white, compact quartz of a scaly fracture and horny lustre, containing often imbedded diallage, hornblende, pyroxene or felspar; sometimes the rock is nearly homogeneous, but at other times angular grains of transparent quartz show clearly its conglomerate character. This rock accompanies the serpentine throughout, and constitutes a range of mountain peaks, one of which, Orford Mountain, is more than four thousand feet above the sea. Beyond this still, on the line of section is a band of impure dolomite, which farther north in the strike is replaced by soapstone, magnesite, and serpentine; a similar band is seen again after an interval of a mile filled with gray slate and the corneous rock.

To these rocks follow gray fossiliferous limestones interstratified with calcareous slates, which form apparently two narrow parallel troughs, one on each side of Lake Memphremagog. On the east side, at Georgeville, they are followed by gray and black glossy slates, and then by talcose and chloritic slates, often micaceous and associated with quartzose beds, and others very talcose; in the strike of these upon the lake, appears a band of serpentine, followed by fine silicious talcose slates. From the position of these rocks, there appears evidence of a great dislocation which has divided the fossiliferous troughs and brought up the corneous rock in a mountain mass on the west side of the lake. Evidence of an anticlinal in this line is found in the dip of the fossiliferous limestones near the quartzose rocks farther on in the strike. Beyond these rocks, east of Georgeville, highly crystalline limestones appear, which however still display fossils that admit of identification.

The remaining twenty miles of the section to the Connecticut exhibit these crystalline micaceous limestones, interstratified with soft micaceous slates; the calcareous beds predominate for a few miles, but the calcareous matter finally gives place to silicious, and the slates become stronger. Some of the more argillaceous beds contain chiastolite, and others exhibit hornblende and garnets. The limestones are all more or less micaceous, and often very crystalline; some are quite white, while others are grayish or blackish. Even the most crystalline present on their weathered surfaces the forms of encrinal discs and corals; and in several the characteristic *Favosites gothlandica*, with various species of *Porites and Cyathophollum* are observed. At Dudswell, in the line of the strike, the fossiliferous beds are finely exposed, and upon the Rivière de Famine, the rock, which is here less crystalline, exhibits, besides these fossils, the *Atrypa affinis*. The fossiliferous beds appear to be near the base of the formation. The rocks of this valley, south-east of the corneous range, are often pierced with masses of a fine-grained, beautiful granite, which forms large dykes and often considerable areas, displacing the calcareous formation. A range of granite-topped hills bounds the valley on the south-east, to the sources of the Chaudière, and constitutes the height of land.

The facts which we have stated, seem to show that the sandstones and red slates, with their chromifero-chloritic bands, are identical with the dolomites, chloritic, and quartzose rocks of Sutton valley; and these with the serpentines and quartzose rocks of the valley of the Missisquoi; so "that the whole of the Green Mountain rocks, including those containing the auriferous quartz veins, belong to the Hudson River group, with the possible addition of a part of the Shawagunk conglomerates." The fossiliferous rocks of the St. Francis valley are evidently Upper Silurian, and referable to the Niagara limestones; a similar formation has been met with at Gaspé and traced one hundred and fifty miles S. W., and from the similarity of the Notre Dame to the Green Mountains and the fact that the Hudson River rocks are continuous along the St. Lawrence to Cape Rosier, we may conclude that the Upper Silurian rocks will be found continuous, or nearly so, throughout. They constitute the calcareo-micaceous formation of Prof. Adams, which he has traced nearly to the southern line of Vermont. Resting upon this formation in Gaspé is a body of arenaceous rocks, seven thousand feet thick, which apparently correspond to the Chemung and Portage group of New York,

with the old red sandstones. As this formation is found extending quite to the Mississippi, it is probable that it will accompany the Silurian rocks through New England, surrounding the coal fields of New Brunswick and of Eastern Massachusetts and Rhode Island. To this may perhaps be referred in part, the rocks of the White Mountains, which may sweep around the western border of the Massachusetts anthracite formation, until lost under the super-carboniferous rocks of the Connecticut River. The limestones of western New England seem to be no other than the metamorphic Trenton limestones of Phillipsburg, while the chlorito-epidotic rocks and serpentines of Sutton valley appear again in the rocks of southern Connecticut between these limestones and the new red sandstone. With such a key to the structure of the metamorphic rocks of New England and of the great Appalachian chain of which these form a part, we may regard the difficulties that have long environed the subject as in a great degree removed, and the bold conjectures as to their metamorphic origin which have been from time to time put forth, fully vindicated.

SEDIMENT OF THE MISSISSIPPI RIVER.

Lieut. Maury said a committee was appointed at the last meeting to obtain further information respecting the Sediment of the Mississippi River. He had requested the Secretary of the Navy to direct the corps at the Memphis Navy Yard, to make the observations necessary, which request he promptly complied with, and he had the report of Robert A. Marr, Esq., of the U. S. Navy, the officer under whose direction the examination was made. The report was adopted and ordered to be printed.

Letter of Lieut. Maury to Hon. J. Y. Mason, Secretary of the Navy at Washington.

National Observatory,
Washington, Dec. 27th, 1848.

Sir, — At the last meeting of the American Association for the Promotion of Science, a committee was appointed to report as to the practicability of reclaiming the drowned lands of the Mississippi Valley, and also as to the probable effect which the reclaiming of those lands would have upon the navigation of that river.

These are subjects of vast national importance, and being such, they need no argument to present them to your favorable consideration.

With the view of aiding this committee in its investigations, I have the honor to request that the officers of the Memphis Navy Yard, be directed to conduct a series of observations with a view to obtain answers to the following questions:—

1st. What is the average quantity of water taken up at Memphis by evaporation?

2nd. What is the average volume of river water that daily passes by that place?

3rd. What is the quantity of silt contained in that water?

4th. What is the daily rise and fall of the river?

5th. What at the different stages of the river, is the rate of the current at the surface and successively at the depth of every ten feet?

6th. What the daily temperature of the water at the surface—and what at the bottom of the main channel way?

7th. And if not already the subject of observation, what is the quantity of rain that falls there?

Satisfactory answers to these questions would throw much light upon the subject in hand, and it is believed that the officers of the Navy already at the Yard would willingly undertake the requisite observations.

Therefore in addition to their regular duties, should you find it not incompatible with the public interests to direct such a series of observations, and should it be desired, I will with pleasure afford any explanation that may be required as to the manner of making these observations.

Respectfully, &c.,

(Signed,) M. F. MAURY.

Hon. J. Y. Mason, Secretary of the Navy, Washington.

Report of Observations made at the Navy Yard at Memphis, during the Months of April, May, June, and a part of July, 1849. By Robert A. Marr, U. S. Navy.

Evaporation. For ascertaining the amount of water evaporated at this place, a box was made of thick plank, painted white inside and out, and filled with water, exposing a surface of two square feet. The average quantity of water evaporated from this box has been $\frac{12}{100}$ of an inch daily. This evaporation should be regarded only as the measure of evaporation from the river and other collections of water of considerable depth.

Rain. A rain gauge received from the Observatory at Washing-

ton has been used for ascertaining the quantity of rain. The average quantity falling has been $\frac{11}{100}$ of an inch daily. The weather has been thought unusually wet.

Quantity of water passing this place. For ascertaining the quantity of water which daily passes this place, I selected a point of the river where there was no eddy current, or overflow on either side — measured the width of the river at high water — sounded — estimated the area of a vertical cross section of the river — divided this cross section into three parts, as is shown in the accompanying plot — ascertained the average velocity of current, (per hour) in each of these divisions — multiplied the area of these divisions of the cross section (reduced according to the height of the river) into the average velocity expressed in feet — added together these products, and multiplied their sum by twenty-four. The average quantity of water passing daily since these observations were commenced has been 57165356160 cubic feet. The river for the last few months has been unusually high, but considering this quantity as an average, the water passing by in one year would amount to 20865354998400 cubic feet — a quantity sufficient to cover an area of 100,000 square miles to the depth of seven and a half feet.

Surface Currents. The surface currents have been ascertained by anchoring the boat and using a line and chip after the usual *principle*, (using the statute mile.)

Currents below the Surface. For ascertaining the velocity of the current below the surface, it was suggested that blocks of wood loaded to sinking, attached by lines of requisite lengths to floats of just sufficient buoyancy to keep themselves on the surface of the water should be placed in the current, and the motion of the floats noticed as indicating the velocity of the current below. This principle of operating has been adopted with slight variations; but owing to peculiarities of the current of the Mississippi river, there are practical difficulties attending this mode which render the results less accurate than is desirable. Owing to variation in direction of current, it is necessary to have a comparatively large float.

This float being influenced by the surface current and by the wind so materially affects the motion of the block below, that I have found it advisable to attempt measuring the velocity of the current beneath the surface only when there was no wind. Sometimes the block appears to take an undulatory motion, causing the float, if small, to disappear at intervals, so that when the block is intended to be sus-

pended at a particular point, there is no certainty, if operating near the bottom, that the block does not sometimes touch the ground, whilst at other times it may be far above, since the strain on the line causes the block to ascend until certain forces acting are counterbalanced. I have attempted to obviate some of these difficulties, and have partially succeeded. For the loaded block I substituted a tin vessel, (filled with water to sinking,) the specific gravity of which is easily graduated very nicely; and is altogether much more easily handled than the block. Using a comparatively large float, I estimated the area of its surface acted upon by the current, and that of the submerged tin vessel; then knowing the rate of the surface current, and observing the motion of the float I estimated the velocity of the current below, instead of taking the velocity of the float as its index, thus, —

Letting $a =$ area acted upon by surface current.
$b =$ velocity of surface current.
$c =$ area of tin vessel acted upon by current.
$m =$ observed velocity of float attached to tin.
$x =$ current below.

then $ab + cx = m(a + c) - ab.$

$$x = \frac{m(a + c) - ab.}{c} = \text{current below.}$$

There is so little difference in velocity between the current at the surface and at the depths of ten, twenty, thirty, and forty feet, that in order to ascertain it, so great a distance would be passed over that too much time would be lost in pulling up against the current of the *Mississippi*. It is necessary for the boat to keep near the float, so that the observer may distinguish it. This renders it necessary that the operations for the current at the depths of every ten feet should be conducted in succession. To do so daily would occupy nearly the whole time of those thus engaged. Under these circumstances, I have only observed the rate of current on the surface and near the bottom, and find the velocity of the current near the bottom to be to that at the surface as 268 is to 300. I believe that very near the bottom it is much less. It appears that the greatest velocity of current obtains when the river is rising most rapidly, and that the rapidity of the current at high water is to that at its lowest stage during the time of these observations about as 33 to 28.

It is impossible to observe the current of this river and notice its effects upon the banks, without being strongly impressed with the

vital importance which the serpentine course of this river (at which travellers often so inconsiderately complain) is to its navigability. The winding course of this river, by diminishing the ratio of its fall to its length, greatly lessens the velocity of current; but the greatest restraint upon the fury of this current arises from the abrupt turns in the channel, which, by forcing the stream from a straight course, constitute a series of checks, without which this river would rush on with such impetuous violence as to render its navigation utterly impracticable.

Temperature of water at the bottom of the river. I have found the temperature of the water at the bottom of the river to be the same as that at the surface; at least, the difference is not observable with the ordinary thermometer. This result being unexpected, I have taken pains to conduct this observation with the greatest care. An oblong box, with a valve at the top and bottom, was used for bringing water from the bottom of the river. The openings at the bottom and top of the box were as large as possible, (consistent with the effective operation of the valves,) so as to permit the water to pass freely through as the box descended, and the valves were loaded with lead, so that they would not be raised by their own buoyancy as the box was hauled up, and yet would readily yield to the resistance of the water as it was lowered. Care was taken that the box retained none of the warmth of the atmosphere. To test this observation more thoroughly, I also used an apparatus for bringing up the bottom water, constructed in the following manner: — Two circular boards were connected together by leather, (bellows fashion,) the upper board having a line attached to it, and the lower one a weight sufficient for sinking the apparatus. When about to use this instrument, the two boards are pressed closely together, so as to exclude the air and surface water, and being confined in this manner by a thread, the apparatus is gently lowered. When it has reached the bottom, by giving the line a jerk the thread is broken, and the apparatus distended and filled with water, flowing through an orifice in the upper board. This arrangement appears to operate perfectly and gives the same result as the box with valves. This equality of temperature may be attributed to two causes. One is, that the sun does not in a short time affect the water *immediately* beneath the surface to a degree observable with the common thermometer. This latter fact is evident, because whenever I have tried the experiment, I have found the river water as warm at midnight as at noon. Another cause may be, that the bottom and surface water,

owing to the great agitation of the current, frequently change positions.

The water is certainly affected by the temperature of the air, but the effect is very gradual.

The fact that the temperature of this river is so little affected by the casual and local variations of the temperature of the atmosphere, suggests the idea that it may be regarded as the mean result of innumerable observations, taken in every portion of the country through which this river flows; and that if the temperature of all the large rivers and other large bodies of water should be carefully noted and combined, the result would indicate the average temperature of the whole country. If such observations were continued for years, they would show whether the general temperature of the country was increasing or diminishing; and whether the spring of any particular year was backward or in advance. This knowledge would be interesting to the philosopher, and might be of practical benefit to agriculturalists.

Should there be occasion to make any farther observations relative to the temperature of this river, and of its rise and fall, I would suggest that with very little trouble, arrangements might be made for having them conducted at several of the wharf boats between New Orleans and Cairo. This would make the observations much more complete.

Overflow. No attempt has been made to ascertain the quantity of overflow, for reasons which I will presently state. In the mean time being aware that the object for which these observations were ordered to be conducted, render a knowledge of the quantity of overflow, not only here but throughout the entire length of the river, extremely desirable, I will remark that the difficulty arising from a want of this information may be at least partially removed, by conclusions to be drawn from the data we already possess, or which may hereafter be readily obtained. The lowest stage of the river since these observations were commenced, has been 20.2 feet above low water. At that point the average velocity of the central surface current was $2\frac{7}{8}$ miles per hour; at the height of 33.6 feet, it was $3\frac{3}{4}$. When the river is at 33.6 feet above low water, there is about one half more water in the river than when at 20.2 feet. Now I think it may be safely assumed, that if all the overflow were confined to the river, the height of the river would not be increased more than fourteen feet, and this increase (reasoning from the data above,) would not give a greater velocity of current than five miles.

It is impracticable for the officers of this yard, in addition to their regular duties, to ascertain the amount of overflow. A single glance at the preliminary steps which would be necessary will render this obvious. It would be necessary to ascertain the area overflowed — the average depth of the water — what portion of it has a progressive motion with the course of the river, and what part of the overflow is exposed to the sun. I will here remark, that the quantity of overflow taken up by evaporation, during the *time* the overflow is *being measured*, is an important item; for it must be remembered, that if the overflow were confined within the banks of the river, there would be but little more water evaporated from the river than before. This remark refers only to the amount evaporated and absorbed by the earth during the time the water flows from the river, because it is only this quantity which would not be embraced in the *measurement* of the overflow.

Silt. The quantity of silt has been ascertained by daily placing a known quantity of river water in a box, drawing off the water as it becomes clear, and weighing (when dried) the earth thus deposited. The average quantity of earth contained in one hundred cubic feet of river water, is twelve and seven tenths pounds. Which gives 26580008400 pounds, or 2137061974 cubic feet of earth passing in one year — a quantity sufficient to make a bed of earth one mile square and seventy six feet deep.

ROBERT A. MARR, *Acting Master*, *U. S. N.*

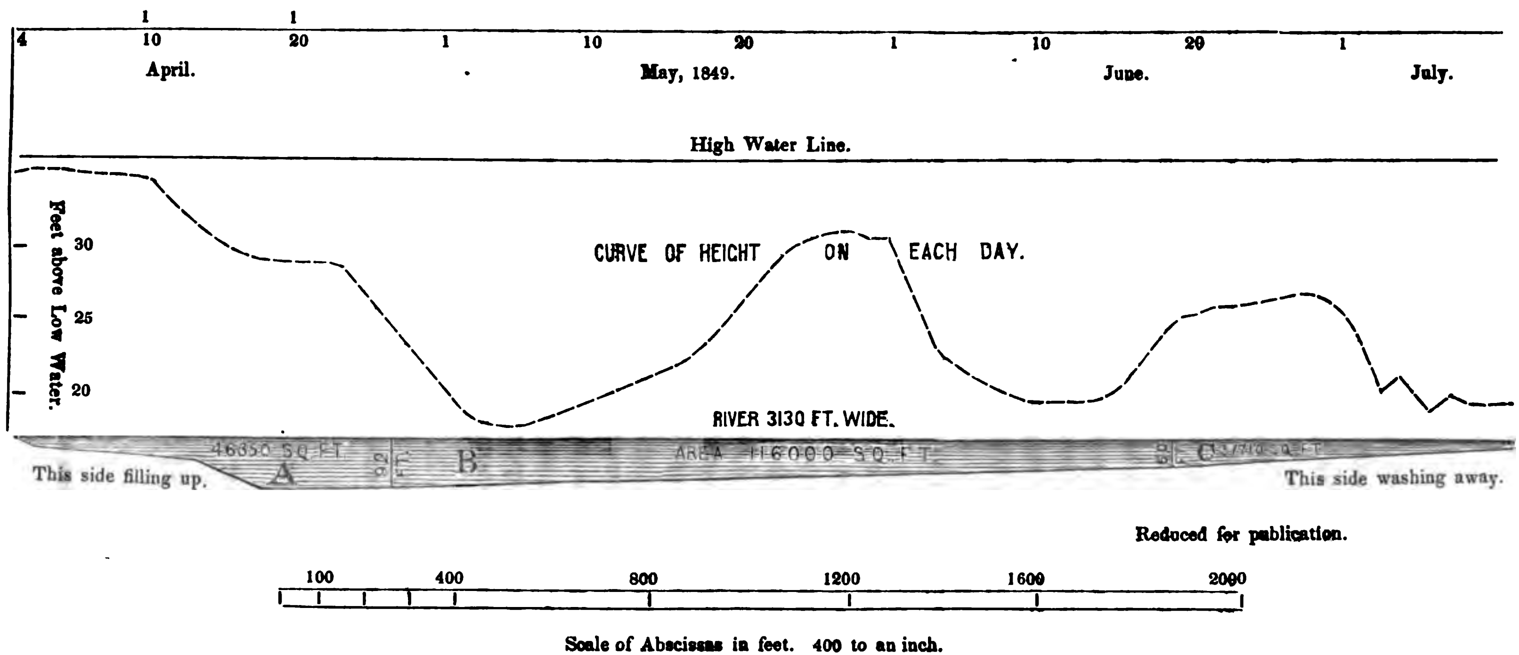
April.	Height of River above low water mark.	Velocities of Surface Currents in miles and eighths.			Mean of average Surface and bottom Currents in miles and tenths.			Areas of A.	Areas of B.	Areas of C.	Quantity of water passing daily, expressed in cubic feet.	Temp. of air at noon.	Weekly Evaporation.	Rain.
	Feet.	A′	B′	C′	A″	B″	C″							
1	33.6	2.0	3.4	2.4	1.8	3.2	2.3	46034	115360	31418	66436077312	64°		
2	33.6	1.6	3.4	2.2	1.5	3.2	2.0	46034	115360	31418	64075004928	73°		
3	33.5	1.6	3.4	2.2	1.5	3.2	2.0	45955	115260	31345	63975605760	70°		60-100 in.
4	33.6	2.0	3.2	2.2	1.8	3.0	2.0	46034	115360	31418	62318006784	68°	9-10 in.	
5	33.6	1.6	3.2	2.2	1.5	3.0	2.0	46034	115360	31418	61151321088	68°		
6	33.5	1.6	3.2	2.2	1.5	3.0	2.0	45955	115200	31345	61055976960	88°		
7	33.5	2.0	3.2	2.4	1.8	3.0	2.3	45955	115200	31345	63412272000	90°		50-100 in.
8	33.4	2.0	3.2	2.4	1.8	3.0	2.3	45876	115240	31272	63312150528	86°		
9	33.2	1.6	3.2	2.2	1.5	3.0	2 0	45718	114720	31126	60769944576	82°		
10	33.0	1.6	3.4	2.0	1.5	3.2	1.8	45560	114400	30980	62693452800	70°		20-100 in.
11	32.5	2.0	3.2	2.0	1.8	3.2	1.8	45070	113600	30615	60449621760	79°	9-10 in.	
12	31.6	2.0	3.4	2.2	1.8	3.2	2.0	44372	112160	29958	63194959872	88°		
13	30.8	1.6	3.2	2.2	1.5	3.0	2.0	43758	110880	29374	58468709376	70°		
14	30.1	1.6	3.2	2.2	1.5	3.0	2.0	43226	109760	28863	57805558272	58°		
15	29.7	1.6	3.0	2.2	1.5	2.8	2.0	42925	109120	28571	54661685760	58°		
16	29.4	1.6	3.0	2.2	1.5	2.8	2.0	42700	108640	28352	54390251520	44°		
17	29.3	2.0	3.0	2.4	1.8	2.8	2.3	42625	108480	28279	56166574464	72°	1 1-10 in.	
18	29.3	1.6	3.0	2.4	1.5	2.8	2.3	42625	108480	28279	55086286464	59°		
19	29.0	2.0	3.0	2.4	1.8	2.8	2.3	42400	108000	28060	56169653760	60°		
20	29.0	2.0	3.0	2.4	1.8	2.8	2.3	42400	108000	28060	56169653760	66°		
21	28.8	1.6	3.0	2.2	1.5	2.8	2.0	42250	107680	27914	53847383040	70°		30-100 in.
22	28.3	2 0	3.2	2.4	1.8	3.0	2.3	41882	106880	27549	58213938816	75°		
23	27.3	2.0	3.2	2.4	1.8	3.0	2.3	41149	105280	26819	57225725568	80°		
24	26.6	2.0	3.2	2.4	1.8	3.0	2.3	40642	104160	26308	56535367680	62°		
25	25.6	2.0	3.4	2.6	1.8	3.2	2.5	39926	102560	25578	58798561536	74°	7-10 in.	
26	24.3	2.0	3.4	2.6	1.8	3.2	2.5	39010	100480	24629	57445534080	86°		
27	23.1	2.0	3.4	2.6	1.8	3.2	2 5	38179	98560	23753	60524221824	82°		
28	22.0	2.0	3.4	2 4	1.8	3.2	2.3	37430	96800	22950	54479235680	74°		20-100 in.
29	21.2	1.6	3.2	2.2	1.5	3.0	2.0	36894	95520	22366	69677654528	78°		
30	20.8	1.6	3 0	2.4	1.5	2.8	2.3	36628	94880	22074	47524942080	65°		
	From high to low water mark measures 34 feet.	Before being combined with the bottom Currents, these velocities were separately reduced to the average.												$1\frac{80}{100}$

May.	Height of River above low water	Velocities of surface Currents in miles and eighths per hour.			Mean of average surface and bottom currents, in miles and tenths.			Areas of A.	Areas of B.	Areas of C.	Quantity of water passing daily, expressed in cubic feet.	Tempt. of air at noon.	Tempt. of River at surface.	Tempt. of River at bottom.	Weekly Evaporation.	Rain.
	Feet.	A'	B'	C'	A''	B''	C''								Inch.	Inch.
1	20.4	2.0	3.0	2.6	1.8	2.8	2.5	36364	94242	21782	50656880384	70°	62°	62°		
2	20.2	2.0	3.0	2.6	1.8	2.8	2.5	36232	93920	21636	48442977792	70°	62	62	1 2-10	33-100
3	20.2	2.0	3.0	2.4	1.8	2.8	2.3	36232	93920	21636	47894635008	80°	63	63		
4	20.2	2.0	3.0	2.4	1.8	2.8	2.3	36232	93920	21636	47894635008	72°	63½	63½		
5	20.4	1.6	3.0	2.2	1.8	2.8	2.0	36364	94240	21782	47252772864	68°	64	64		
6	20.5	1.2	3.0	2.4	1.5	2.8	2.3	36430	94400	21855	47250656640	68°	65	65		
7	20.9	1.4	3.2	2.4	1.1	3.0	2.3	36694	95040	22147	47700132480	82°	66	66		10-100
8	21.0	1.6	3.0	2.2	1.4	2.8	2.0	36760	95000	22220	45931438080	78°	66	66		80-100
9	21.5	2.0	3.0	2.2	1.5	2.8	2.0	37095	96000	22585	47307363840	64°	66	66	9-10	
10	21.9	2.0	3.2	2.4	1.8	3.0	2.3	37363	96640	22877	51928652160	72°	66	66		
11	22.2	2.0	3.2	2.4	1.8	3.0	2.3	36894	97120	23096	52067980800	80°	66	66		
12	22.4	2.0	3.0	2.4	1.8	2.8	2.3	37028	97440	23242	49790854080	80°	66½	66½		
13	22.7	2.2	3.2	2.4	1.8	3.0	2.3	37229	97920	23461	52554902400	80°	66½	66½		
14	23.0	2.2	3.6	3.0	2.0	3.4	2.8	38110	98400	23680	60456084480	86°	67	67		
15	23.5	2.2	3.6	3.0	2.0	3.4	2.8	38455	99200	24045	61017707520	86°	67	67		
16	24.9	2.2	3.6	3.0	2.0	3.4	2.8	49360	101440	25067	62790792192	80°	67	67	9-10	
17	26.0	2.0	3.4	2.6	2.0	3.2	2.5	40210	103200	25870	70234451200	76°	67	67		
18	27.1	2.0	3.2	2.4	1.8	3.0	2.3	41003	104960	26673	57169286016	78°	67	67		
19	28.0	2.0	3.2	2.4	1.8	3.0	2.3	41660	106400	27330	58059997440	78°	67	67		
20	28.8	1.6	3.2	2.2	1.8	3.0	2.0	42252	107680	27914	47812386072	84°	67½	67½		40-100
21	29.3	2.0	3.2	2.2	1.5	3.0	2.0	42625	108480	28279	57194887680	88°	67½	67½		30-100
22	29.8	2.0	3.4	2.4	1.8	3.2	2.3	43005	109280	28644	62471211264	86°	68	68		
23	30.3	1.6	3.4	2.4	1.8	3.2	2.3	43378	110080	29009	62987075712	88°	68½	68½	1	80-100
24	30.3	2.0	3.4	2.4	1.5	3.2	2.3	43378	110080	29009	61888703680	76°	69	69		40-100
25	30.3	2.0	3.4	2.4	1.8	3.2	2.3	43378	110080	29009	62983075712	76°	69	69		
26	29.4	2.0	3.4	2.4	1.8	3.2	2.3	42700	108640	28352	62057014272	78°	69½	69½		
27	29.4	2.2	3.6	2.6	1.8	3.4	2.5	42700	108640	28352	65528939520	82°	69½	69½		
28	28.6	2.2	3.6	2.6	2.0	3.4	2.5	41964	107360	27768	65688159840	78°	70	70		
29	27.4	2.2	3.6	2.6	2.0	3.4	2.5	41222	105440	26892	64395302400	84°	70	70		
30	26.1	2.2	3.6	2.6	2.0	3.4	2.5	40282	103360	25942	62960261760	88°	70	70	1 4-10	
31	24.7	2.2	3.6	2.6	2.0	3.4	2.5	39290	101120	24921	61419980160	86°	70½	70½		33-100
		Before being combined with the bottom currents, these velocities were separately reduced to the average.														$3\frac{46}{100}$

June.	Height of River above low water mark.	Velocities of surface Currents in miles and eighths per hour.			Mean between the average surface and bottom currents.			Areas of A.	Areas of B.	Areas of C.	Quantity of water passing daily measured by cubic feet.	Temperature of air at noon.	Tempt. of water at surface.	Tempt. of River at bottom.	Weekly Evaporation.	Rain.
	Feet.	A′	B′	C′	A″	B″	C″								Inch.	Inch.
1	23.5	2.0	3.4	2.4	1.8	3.2	2.3	38455	99200	24045	56005488000	84°	71°	71°		
2	22.8	1.6	3.0	2.2	1.5	2.8	2.0	37974	98080	23534	48464114688	92°	71½	71½		
3	22.4	1.4	3.0	2.2	1.4	2.8	2.0	37695	97440	23242	47151720304	80°	71½	71½		
4	21.9	1.4	3.0	2.2	1.4	2.8	2.0	37363	96640	22877	46715960224	84°	72	72		
5	21.4	1.4	2.6	2.0	1.4	2.5	1.8	37028	95840	22512	44366232576	88°	72	72	1	20-100
6	21.0	1.2	2.6	2.0	1.2	2.5	1.8	36760	95200	22220	42636503040	84°	72	72		
7	21.0	1.2	2.6	2.0	1.1	2.5	1.8	36760	95200	22220	42636503040	90°	72½	72½		90-100
8	21.0	1.2	2.6	2.0	1.1	2.5	2.0	36760	95200	22220	42636503040	88°	73	73		30-100
9	21.0	1.2	2.4	2.2	1.1	2.3	2.0	36760	95200	22224	37938954240	84°	73½	73½		40-100
10	20.8	1.4	2.4	2.2	1.4	2.3	2.0	36628	94880	22074	39745780064	85°	73½	73½		130-100
11	20.9	1.4	2.6	2.2	1.4	2.5	2.3	36694	95040	22147	44512376832	88°	74	74		
12	21.1	1.4	3.0	2.2	1.4	2.8	2.5	36827	95360	22293	46018596096	90°	74	74		
13	21.7	1.6	3.0	2.4	1.5	2.8	2.3	37229	96320	22781	48349217664	88°	74½	74½	1 1-10	
14	23.0	2.0	3.2	2.6	1.8	3.0	2.0	38110	98400	23680	53602306560	92°	75	75		
15	24.5	2.0	3.4	2.4	1.8	3.2	2.0	39150	100800	25775	57025584000	80°	75	75		
16	25.4	2.0	3.2	2.2	1.8	3.0	2.0	39784	102240	25432	54765002304	92°	75½	75½		
17	25.4	2.0	3.0	2.2	1.8	2.8	2.0	39784	102240	25432	52036421184	96°	76	76		
18	25.8	1.4	3.0	2.2	1.4	2.8	2.0	40068	102880	25724	40254864384	96°	76	76		
19	25.8	1.4	3.0	2.2	1.4	2.8	2.0	40068	102880	25724	40254864384	92°	76	76		
20	25.8	1.4	3.0	2.2	1.4	2.8	2.0	40068	102880	25724	40254864384	98°	76	76	9-10	
21	26.0	1.4	2.6	2.2	1.4	2.5	2.0	40210	103200	25872	50307087680	84°	76¼	76¼		30-100
22	26.4	1.4	2.6	2.4	1.4	2.5	2.3	40498	103840	26163	50198413056	84°	76¼	76¼		30-100
23	26.5	1.6	2.6	2.2	1.5	2.5	2.0	40570	104000	26235	50317847040	89°	76¼	76¼		13-100
24	26.5	1.6	3.0	2.2	1.5	2.8	2.0	40570	104000	26235	51775511040	92°	76½	76½		2-100
25	26.3	1.6	3.0	2.2	1.5	2.8	2.0	40426	103680	26729	51595771392	94°	76½	76½		2-100
26	25.8	1.6	2.6	2.2	1.5	2.5	2.0	40068	102880	25729	49704861696	96°	76½	76½		
27	25.3	1.6	3.0	2.4	1.5	2.8	2.3	39713	102080	25359	57662540160	94°	76½	76½		44-100
28	24.5	2.0	2.4	2.6	1.8	2.3	2.5	39156	100800	24775	51653481600	88°	77	77		
29	23.7	1.6	3.2	2.4	1.5	3.0	2.3	38593	99520	24191	52708943232	94°	77	77		
30	21.3	2.0	3.0	2.6	1.8	2.8	2.5	36961	95680	22439	50088126331	90°	77½	77½		2-100
																$4\frac{3}{100}$

Before being combined with the bottom currents, these velocities were separately reduced to the average.

July.	Height of River above low water mark.	Velocities of surface Currents in miles and eighths per hour.			Mean of average surface and bottom currents, in miles and tenths.			Areas of A.	Areas of B.	Areas of C.	Quantity of water passing daily, expressed in cubic feet.	Tempt. of air at noon.	Tempt. of River at surface.	Tempt. of River at bottom.	Weekly Evaporation.	Rain.
	Feet.	A'	B'	C'	A''	B''	C''								Inch.	Inch.
1	21.9	2.2	3.2	2.6	2.0	3.0	2.5	37360	96640	22877	53454614400	90°	77½	77½		
2	21.3	2.2	3.0	2.4	2.0	2.8	2.3	36961	95680	22439	49856171904	90°	78	78		
3	20.3	2.2	3.2	2.4	2.0	3.0	2.3	36298	94080	21709	51292036224	94°	78½	78½		
4	21.3	2.0	3.0	2.2	1.8	2.8	2.0	36760	95200	22220	47794728960	90°	79	79	1	
5	20.6	1.6	2.6	2.2	1.5	2.5	2.0	36496	94560	21928	42913677312	84°	79	79		20-100
6	20.7	2.0	3.0	2.4	1.8	2.8	2.3	36562	94720	22001	48360332928	86°	79½	79½		80-100
7	20.8	2.0	2.6	2.6	1.8	2.5	2 5	36628	94880	22074	45405727488	96°	79½	79½		50-100
8	21.0	2.0	3.0	2.4	1.8	2.8	2.3	36760	95200	22220	48639444480	88°	79½	79½		
9	21.5	1.6	3.2	2.4	1·5	3.0	2.3	37095	96000	22585	50598979200	92°	79½	79½		
10	22.2	2.0	3.0	2.6	1.8	2.8	2.5	36894	97120	23096	50191916544	98°	79	79		17-100
11	22.2	2.2	3.0	2.4	2.0	2.8	2.3	36894	97120	23096	50541613056	94°	79	79	6-10	4-100
12	22.6	2.2	3.0	2.4	2.0	2.8	2.3	37838	97760	23388	5009305780	94°	79½	79½		
13	22.9	2.0	3.2	2.4	1.8	3.0	2.3	38042	98240	23607	55302148224	98°	79½	79½		27-100
14	23.0	1.6	3.0	2.2	1.5	2.8	2.0	38110	98400	23680	48642232320	84°	79½	79½		50-100
15	23.1	1.4	3.0	2.2	1.4	2.8	2.0	38179	98560	23753	47754885312	80°	79½	79½		$2\frac{48}{100}$



4
1
10
April.
1
20
1
10
May, 1849.
20
1
10
June.
20
1
July.
High Water Line.
Feet above Low Water.
30
25
20
CURVE OF HEIGHT ON EACH DAY.
RIVER 3130 FT. WIDE.
This side filling up.
A
B
AREA 116000 SQ. FT.
This side washing away.
Reduced for publication.
100
400
800
1200
1600
2000
Scale of Abscissas in feet. 400 to an inch.

Sixth Day, August 20, 1849.

SECTION OF NATURAL HISTORY, ZOÖLOGY, &c.

PROF. S. S. HALDEMAN was called to the chair.

HISTORY OF PHALANGOPSIS, A GENUS OF ORTHOPTERA, WITH THREE NEW SPECIES, TWO OF WHICH FORM A NEW SUB-GENUS. BY S. S. HALDEMAN.

THE sub-genus proposed is named *Daihinia*, and differs from *Phalangopsis* proper, by having shorter attennæ, shorter and more robust limbs, and the second and third joints of the tarsi equal, characters which approximate it to *Stenopelmatus*. The males of all the American species of both forms differ from the females, in having longer limbs, and a row of strong spines upon the under surface of the femora. The following North American species are indicated: —

PH. (D.) BREVIPES, *Hald.* Dark brown, mottled with lighter shades, all the *tibiæ shorter than the femora*, strongly spinous, anterior ones semi-fossorial, tarsi trimerous, medial ones tetramerous, femora of the male dilated with long spines upon the *outside* of the inferior margin. Long. 22 millims.: posterior femora of the male, 14; female, 12; posterior tibiæ of the male, 13; of the female, 9; tarsi of the male, 4; of the female, 3½.

From the vicinity of the river Platte. In the collection of Prof. AGASSIZ.

PH. (D.) ROBUSTUS, *Hald.* Uniform dark fuscous dorsum and upper surface of the femora sparsely scabrous; feet rather short; posterior femora of female very robust, rather suddenly contracted inferiorly at the extremity; inferior margin with a row of robust spines upon the *inside*; tarsi tetramerous. Length thirteen lines.

PH. GRACILIPES, *Hald.* Shining yellowish brown, mottled; colors arranged transversely; limbs larger than in any other American species, the posterior femora of the male being twelve, and the tibiæ thirteen lines, in a specimen ten lines long. This species is from central Pennsylvania, and is nearest allied to PH. LAPIDICOLA, *Burm.*, which extends from Eastern Pennsylvania to Georgia. PH. MACULATA, *Harris* is allied to the latter, but it has the posterior tibiæ in the male waved at the base.

On the Larva of Physocœlus inflatus. By S. S. Haldeman.

This larva resembles that of Tenebrio, except that the mandibles have four terminal teeth and a basal molar; the termination of the abdomen is sloped off above at an angle of about forty-five degrees, the surface depressed into a spoon-shaped cavity. It inhabits woods under stones and rubbish.

On the Brachiopoda of the Silurean Period; particularly the Leptænidæ. By James Hall.

(This paper was introduced as a portion of a Report on the classification of the Brachiopoda of the Palæozoic period.)

This natural group of the *Brachiopoda*, the *Leptænidea*, first included among the *Anomiæ* as Anomites, and subsequently under the genus *Productus* by Mr. Sowerby, has become so numerous, and, in geological investigations, so important, that an enumeration of its distinctive features is necessary. For a long time, all the fossil shells with a straight hinge line, having one valve convex and the other concave, and more or less abruptly curved towards the margin, were called *Productus*. Dalman proposed the name *Leptæna* to include certain of these, but either from not understanding his characters or from other causes, the difference between *Leptæna* and *Productus* was not recognized. So late as 1839, Mr. J. de C. Sowerby, in the *Silurian System*, introduces the genus *Leptæna*, but without noticing any distinction between it and *Productus*. Prof. Phillips in 1841, (Palæozoic Fossils &c.,) regards it as decided that *Leptæna* will become the favorite term in place of *Productus*.

Von Buch, in 1842 in his work *Über* Productus *oder* Leptæna, has failed to draw any distinction between these genera.

In the United States we have been inclined to use the name *Strophomena* for all the species of this group not possessing the decided characters of *Productus*.

On reviewing the subject more carefully and attending to other parts besides external form, there appears good reason to recognize several distinct genera, each alike worthy of attention. In the mean time several names have been proposed to include certain portions of the group, and attempts have been made to determine the limits of the genus *Leptæna*, *Strophomena*, &c. The name of *Chonetes*, proposed

by Pander, has been generally adopted for certain forms of this group of fossils, while *Strophomena* has been either abandoned, or so variously applied, or misapplied, that it has nearly lost all signification.

In 1846, Mr. King, in the annals of Natural History, proposed to limit the genus *Leptæna* to those forms which are transversely wrinkled, as *L. rugosa*, &c., including under the genus *Strophomena* all the others. Mr. Sharpe (Quar. Jour. Geol. Soc., London, Aug. 1848) proposes to restrict the genus *Leptæna* to such forms as are externally irregularly curved, and internally have the muscular impression limited and defined by the extension and curving of the dental lamellæ.

The genus *Strophomena* would include those more regularly arched or nearly flat, and evenly striated, having the muscular impression limited only on the sides, by the extension of the flat plates of the dental lamellæ. Although not satisfied that the character of the exterior as described by Mr. Sharpe, always corresponds to such internal characters as he notices for the restriction of the genera, I am nevertheless satisfied that such internal characters are of the first importance. Adopting this proposed restriction of the two genera, we have *Leptæna* and *Strophomena* very naturally distinguished.

Both these genera have the hinge line smooth and straight, indented on the dorsal valve by the foramen, which is partially closed, and the line of the ventral valve having a callosity partially filling the foramen in the dorsal valve. The area is mainly on the dorsal valve, and is striated longitudinally. The exterior surface is usually striated more or less equally with rounded striæ, which are crossed by fine concentric striæ. We find, however, other forms which do not meet the requirements of these generic descriptions. Their general external form is similar, but the striæ are usually more sharp, and rise at irregular intervals on the surface; the concentric striæ are less conspicuous or abrupt; the hinge line is crenulated and continuous, showing no foramen or interruption, subsequently closed on the dorsal valve, nor any callosity on the hinge line of the ventral valve, as in *Leptæna* and *Strophomena*. The dorsal area, moreover, is striated transversely as well as longitudinally. In the interior there are no dental lamellæ margining or surrounding the muscular impressions, which are spread out over considerable surface in the dorsal valve, showing partially a double or bilateral arrangement. In the ventral valve there is some indication of a limitation, or marginal elevation, to the muscular impression, but the character is quite distinct from the same in *Leptæna*. For this genus I propose the name *Strophodonta*, from the crenulated

hinge line, which is recognizable even in casts and impressions of the shell.*

The genus *Chonetes* is recognized simply by the character of having a row of small curved spines ornamenting the cardinal line. The striæ are also peculiar and characteristic. The form of the muscular impression in this genus is not well known to me, though a large part of the inner surface of the valve is marked by prominent papillæ in regular rows or lines. The form and character of the muscular impressions are probably as distinctly characteristic as in the other genera.

In the genus *Productus* the general external form is more arched, and the shell more ventricose; the surface is for the most part marked by strong radiating lines and concentric striæ, and very frequently armed with spires of greater or less length.† The interior of the shell shows the muscular impressions not limited by any dental lamellæ, and distinctly arranged on each side of a centre, forming a double muscular impression, and possessing in a higher degree the characters first shown in *Strophodonta*.

These are in brief some of the prominent zoölogical characters on which I would propose sub-divisions in this group; and it is not improbable that other sub-divisions may yet be required.

After having determined the value of these zoölogical characters, we will inquire whether the genera thus distributed have any geological value, or value in reference to time. The importance of vital zoölogical characters among the older fossils, in regard to their geological position, appears not to have attracted due attention. We have used the terms *Leptæna Strophomena*, and *Productus*, without considering the geological position of the fossil. M. Von Buch in his work before cited, has not only failed to draw any distinction of zoölogical characters between *Productus* and *Leptæna*, but has equally failed to notice that they have any importance as indicating priority of time.

The genera *Leptæna* and *Strophomena*, as here restricted, are found in the lower Silurean rocks in great numbers; while *Strophodonta* and *Chonetes* are unknown in the same position. These two latter

* Mr. Sharpe has recognized some of the peculiarities of this genus in *L. demissa* of Conrad, which is a good type of the genus. I had, however, before this separated the genus from *Leptæna* and *Strophomena*, giving the name to M. de Verneuil in 1846, as *Cardiodonta*.

† I believe that the species included in *Productus*, even as now restricted, may be separated into two distinct genera.

genera appear for the first time, each in a single species, at the period of the Clinton group of New York. They however become more abundant in the Hamilton group, where *Leptæna* and *Strophomena* are scarcely known at all. The genus *Productus* makes its first appearance in the corniferous limestone, becoming of more frequent occurrenec in the Chemung group and only reaching its maximum development in the Carboniferous period.

We see in the zoölogical characters a beautiful progressive development, commencing from *Leptæna* as the lowest type. There is a constant increasing number of characteristic features, a constant succession of ornament and appendage, in each successive generic type as it marks the successive periods of time. In *Leptæna*, the earliest form, we see the muscular impression in the centre near the hinge line, and almost surrounded by the dental lamellæ. In Strophomena this muscular impression is less restricted; in *Strophodonta* it has become almost double, being distinctly impressed on each side of the centre; while in *Productus* the separation is complete. This progressive change is too significant to be misunderstood or disregarded. We see the gradual progress from a lower to the expression of a higher type in this single feature, the gradual unfolding of an embryonic type in its successive changes.

Prof. Agassiz remarked upon the new genera introduced by Prof. Hall, that these researches would show how much remained to be done in the zoölogical investigations of the fossil remains. There was no reason for neglecting this work because it was difficult. The gradual progressive development of the types had already been noticed by him in the classes of fishes, crustaceans and echinoderms, and had been found to agree with the embryonic growth of these animals. The distinctions pointed out by Prof. Hall were true, scientific distinctions, and he who would not accept them, because so critical, should not pretend to scientific eminence. Prof. A. alluded to a palæontologist of high rank who had declared these nice differences to be of no value, certainly not because they were not real, but because they clashed with his opinions, and expressed his indignation at the attempt of some scientific men to suppress careful research by the authority of great names.

Prof. Hall remarked upon the differences of structure of animals of different periods, and inquired whether the very existence of two apparently similar types in different geological periods was not in

itself sufficient to lead to the presumption that they were really different species.

On Graptolites, their Duration in Geological Periods, and their Value in the Identification of Strata. By Prof. J. Hall.

The species of this genus already known are with one exception confined to the older Palæozoic, or lower Silurian strata. This exception is the *G. ludencis* of Murchison, which occurs in the Ludlow rocks of England. I have already described fifteen species from the older strata of New York, of which ten are new; five having been identified with forms previously described. Since the publication of these new species, several of them have been found in England in the older Silurian slates.

This genus has been considered as peculiarly Silurian in its character, since no species has yet been recognized in strata of the Carboniferous period. I am inclined to regard it as much more restricted in its geological range, than usually admitted, being typical not only of the Silurian period, but of the lower or older deposits of that period. More than twenty species are known in acknowledged lower Silurian strata; and the genus therefore must be regarded as eminently characteristic of those rocks;—the few known in a higher position being less widely distributed, and very limited in geological range.

In addition to the fifteen species described from the older strata of New York, I have two other species from the rocks succeeding, but still very low in the second period. One of these, the *G. Clintonensis*, I described in my report on the third geological district of New York, in 1843. Subsequent researches have been rewarded by a single additional species only; and in all the collections made in New York, or elsewhere in the United States, I have not been able to find a Graptolite in any position higher than the Clinton group. Therefore, for the rocks of the United States, we may regard the species of this genus as eminently valuable in distinguishing the older strata. The two species of the Clinton group are in shale, immediately above the *Pentamerus oblongus*, which is regarded as a Lower Silurian fossil in Europe; so that we see how closely connected even these species are with Lower Silurian types. The two species of the Clinton group are clearly distinguishable from those of the lower strata, first in *G.*

Clintonensis by the serratures, (which are one side only of the stipe,) being strongly and abruptly recurved at the point, which is not extended into a minute filament like some of the lower species, but terminates abruptly. The other species, *G. venosus* is broad with a central capillary axis and serratures on both sides. The whole substance is finely veined or reticulated like the skeleton of a leaf. This species may very well form the type of a new genus, since we shall, perhaps, find it necessary to sub-divide the species of the present genus. But at this time we have comparatively so little knowledge of the true nature of these fossils, that I have not thought proper to separate this from the others. In the recurved serratures *G. Clintonensis* closely resembles *G. ludensis*, of Murchison; and this character, with the deeper serratures, already distinguishes both these species from *G. sagittarius* of Hisinger.

So long as the geological position of *G. ludensis* remains undisputed, we must admit that single species as occurring in Upper Silurian in England, though the fact, that the genus in the United States is confined to the strata of the earliest period, and the lower portion of the second period only, seems to be well established. The occurrence of a species in a higher position in England, cannot affect the value of our species for the identification of the Utica slate, the Hudson River group, and the shales of the Clinton group.

The facts in regard to their geological distribution afford some ground for speculation, if not for presumptive evidence as to their zoölogical relations. In a large number of instances we are able to trace the connection of a fossil genus or family with an existing one through successive periods; showing in more modern periods a nearer approach to existing forms. This we are not able to do in relation to Graptolites; and I am therefore disposed to question their analogy with the *Sertularidæ* or the *Pennatulidæ*, since we have nothing throughout the higher Silurian, Devonian, or Carboniferous periods, by which their connection can be shown.

On the Bone Caves of Pennsylvania. By Prof. Baird.

It may not be known to all present that the discovery of bone caves in this country is of very recent date. They have been found for many years in Europe; and in France, in great numbers. In this continent there are on record but two cases of this kind; one of them in Canada, and the other in Virginia.

Having been induced, from the resemblance of the cave near Carlisle to the celebrated Kirkland cave, to suspect the existence of bones in it, I commenced an exploration of it some two years ago, and was abundantly rewarded by finding a large number of animal remains. I subsequently examined two other caves in Pennsylvania. The first of these exhibited an entrance on the top of a hill. There was a vertical shaft about thirty feet in length, which opened into a large gallery. My first visit furnished me a skeleton of a bear nearly perfect. It lay upon the surface, and was probably the relic of a bear that had fallen into the cavity within quite a recent period, and had been unable to escape. I visited the same cave again during the last winter, but I found that the main entrance had been entirely filled up with stones, thrown in by the proprietor of the cave, in order to prevent the danger to his cattle of falling in and being killed. I could only get into a narrow cavity, where I obtained nothing of any importance.

The second is on the bank of the Susquehannah, about half a mile below the railroad bridge. Here the entrance is in limestone rock, and —— feet above the river at low water. The opening is a nearly vertical slope, which passes down twenty feet, where it widens. At the bottom is a deposit of mud, in which numerous bones are imbedded. The other end communicates with a bed of water. Here I obtained a few bones, but none of any importance.

The main cave is on the bank of the creek, nearly on a level with the water, the entrance being ten feet high. The floor of the cave is nearly on a level with the extremity, and the cave is nearly three hundred feet in length. Here I obtained some of my specimens, which were generally imbedded in the floor of the cave. It is in the stratum of black mud, about ten inches in depth, that most of the bones are found. This lies above several other strata of deposits, of stalagmite, &c. The bones occur in every imaginable condition of preservation, and of confusion. A great majority, however, are very much broken and gnawed by small *Rodentia* that have inhabited the cave at a certain period.

In addition to this locality, there is another where the most perfect specimens are obtained. There is a series of galleries near the roof of this cave, which can only be reached by ladders, being sometimes eight or ten feet above the floor of the cave. These are filled with mud, and in this mud the bones are distributed. The remains have evidently come in from above, as there is no other possible means by

which they could be filled to this height. The character of these remains is quite interesting in some respects. The number of species of mammalia found there is nearly twice that of present existing species in Pennsylvania. Nearly five per cent. consist of extinct species; the remaining ninety five per cent. are recent. The recent bones are of various species of wolves, foxes, rabbits, bears, muskrats, otters, lynxes, panthers, beavers, &c.

The question may perhaps arise here, as it frequently has before, how these bones got into the cave? Whence this vast accumulation of remains? I say "vast," because I possess of single species of deers, remains that must have belonged to more than one hundred individuals, and I am very far from having cleared out the cave. Various theories are proposed for the production of the bones. Some geologists have supposed that they have washed in from without; others that they have been dragged in by wild beasts; and this latter theory is strengthened by the fact that a great majority of the bones are of the weaker animals, such as would naturally fall the prey of any carniverous animal.

It has appeared to me from the examination of the peculiar circumstances of the cave, that several causes have combined to furnish this accumulation. I can hardly assent to the theory that water has been the means of introducing these bones, for there is no reason why there should be such an accumulation of the bones outside of the cave under any circumstances which would admit of their being washed in. It is quite possible that many of them have been introduced by wild beasts; and some of them bear toothmarks, which were probably from the teeth of the animals which dragged them within the cave and devoured them there. But I am inclined to think that the principal source of this accumulation, is from the sink-holes above, with which these caves connect.

These sink-holes are curious depressions of the soil, found in limestone regions, varying in diameter from ten feet upwards, with an aperture at the bottom through which the water escapes. They are generally overgrown by small bushes, and are just the places to which such animals as the fox and wolf would resort to feed upon an animal just captured. Its bones would be left after the repast, either in the hole or upon the side, until some heavy rain should occur, when the water of the surrounding country, of which these sink-holes are generally the outlet, would carry them down into the cavity. These sink-holes, in almost all cases, communicate with excavations in the

rock or soil beneath; and most of our Pennsylvania caves I believe to have been formed by their action. A rain of unusual violence may close up the inlet into one of these caves and then a new cave will be formed. I have not been able to trace in this cave any communication with the external sink-holes; but I have in other cases, and I have found a little mass of earth at the bottom, and, in many cases, bones introduced there within a few weeks or months, and sometimes even with the cartilage still upon them. There is nothing of that kind in this cave, which is finished. Besides the remains of mammalia, there are numerous remains of other vertebrata — birds in great quantities, particularly wild turkeys, and some of these of an enormous size, probably weighing thirty or forty pounds. There are numerous bones of the swan, several ducks and some large water-birds, whose position I have not had time to ascertain. I have found there the humeri of birds quite as large as the pelican, and probably they are of the pelican itself. Of tortoises, there are eight or ten different species, being found there in great quantities.

The bones of serpents are quite common. I found the lower jaw of a salamander, quite different from any thing we have there now. Also, some remains of fishes, vertebræ and scales mixed up in the mud. But I have not had time to determine many of them. In this mud I found also some very perfectly preserved Indian remains, arrow heads of great beauty, and fragments of pottery. They occur only in the upper two or three inches of this floor.

Prof. B. illustrated the above with sections of the cave, upon the blackboard.

The following extract from a letter was read by Prof. Horsford:

The Discovery of a new Cave in Kentucky. By Dr. Lewis Feuchtwanger, of New York.

This cave was discovered, a few weeks ago in Kentucky, about twelve miles distant from the celebrated Mammoth Cave, and a few miles from Bowling Green, at the mouth of Green River. The discoverer, Mr. S. G. Stevenson, of the latter place, informed me that he had penetrated five miles, where the cave was still extending; and it is more than probable that it will prove the largest cave known. It abounds in a variety of beautiful avenues, and the stalactites, stalagmites and other incrustations, are there found in every form. A few

small samples were inclosed in a letter to me; the one proves, by analysis, to be a sulphate of alumina, and the other a colcothar. The alum does not contain either potassa or ammonia, but forms a double salt with soda: although I have not definitely ascertained the existence of the base (soda,) I have satisfied myself that it does not contain either of the other bases, and from its permanent crystalline condition, it cannot be a pure sulphate of alumina, without a double base — this forming a deliquescent salt. The fact, that the whole country abounds in saline springs, supports the conclusion that it is a soda-alum. Within two miles from the new cave a regular stratum of this soda-alum has likewise been discovered. I am in daily expectation of a collection of specimens, which I requested to be forwarded by express, not alone of all the minerals, fossils, and incrustations, but also of any reptiles or fishes that might be discovered in the cave; and regret much that they have not yet come, in season to be exhibited at the present meeting.

Adjourned.

W. J. BURNETT, *Secretary.*

Seventh Day, August 21, 1849.

GENERAL MEETING.

Prof. Henry in the chair. The Secretary read the proceedings of the last meeting.

The resolutions offered yesterday, recommending the preparation of manuals of scientific research, referred to the standing committee, were reported, with the recommendation that they be adopted.

Dr. Gray thought that the idea of *manuals*, or a *series of volumes*, did not meet the object in view. It was not an extended discussion of particular branches of science, but general principles, as a guide for research in all sciences, in one compact volume.

Pres. Everett said he did not suppose it was intended to require that more than one volume should be prepared, but to leave it for those having the works in charge to determine, that their hands might not be tied.

Prof. Hall said that in the committee it was thought that one volume

might be prepared on physical science and another on natural history. It was not intended to make more than two volumes.

The resolutions were adopted.

Mr. WILLIAM C. REDFIELD, on behalf of the standing committee, offered the following resolution:

Resolved, That a committee be appointed to memoralize the Legislature of Pennsylvania, in reference to the publication of the final Geological Report of that State. Adopted.

The Chairman appointed the following persons as members of the committee: — President HITCHCOCK, of Amherst; W. C. REDFIELD, Esq., of New York; SOLOMON W. ROBERTS, Esq., of Philadelphia; Prof. A. D. BACHE, of Washington, D. C.; Prof. JAMES HALL, of Albany; Prof. H. D. ROGERS, of Boston.

The following resolution, submitted by Dr. BREED, of New York, was reported by the standing committee:

Resolved, That the Association authorize the standing committee to prepare and submit to the public, at the meeting in March next, a circular letter setting forth the great importance to the interests of American education, and of true civilization, of a more general admission of the Physical and Natural Sciences into the systems of school and collegiate instruction throughout our country.

After considerable discussion, in which Dr. HARE, Profs. HENRY, HALL, ROGERS, Pres. EVERETT, and others took part, the resolution was laid on the table, to be taken up at the next annual meeting.

Pres. EVERETT read the report of the committee on the subject of the coast survey, as follows:

REPORT OF THE SELECT COMMITTEE ON THE COAST SURVEY.

The Select Committee of the American Association for the Advancement of Science, to whom was referred the communication of Prof. A. D. BACHE, Superintendent of the United States Coast Survey, relative to the history, progress, and present condition of the same, have had that subject under consideration and submit the following report, in part:

The committee regard the survey of the coast of the United States — an undertaking originally recommended to Congress by President Jefferson — as a work of the highest national importance and utility. It is intimately connected with the advancement of several branches

of science; but in a national point of view it is principally important in reference to the commerce and navigation of the country.

The superiority of the modern over the ancient civilization is in nothing more apparent than in the maritime commerce and navigation of modern times as compared with the ancient. The mariner's compass, the quadrant, and the chronometer, on the one hand, and the astronomical tables which science has furnished to the practical navigator, on the other, are the direct agents by which his superiority has been brought about. But it is quite evident, that, unless the position of the chief points along the coast of commercial countries is accurately laid down, it is of comparatively little consequence, that the navigator is able to lay his course with boldness and precision across the intervening seas. The most serious risks to life and property attending navigation are those which present themselves in making the land; and one of the first objects which have engaged the attention of powerful maritime states, in modern times, has been the survey of their line of coast, and the minute delineation of its headlands, channels, roadsteads, harbors, reefs, shoals, soundings, and currents. A want of accurate information on any of these points may occasion the most deplorable sacrifices of life, and a destruction of property far beyond the cost of a thorough scientific survey.

There is probably no one country in the world, which presents a line of coast accessible to commerce, so extensive and various as the United States; certainly not if we take into consideration our recent acquisitions on the Pacific. Our commerce and navigation are already second to those of Great Britain alone; and in consequence of the peculiar distribution of the great centres of trade along an immense line of geographical extent, our coasting trade bears a higher ratio to the aggregate of our navigation, than in any other country. It is of so much greater importance to us that our coast should be well surveyed and laid down; and if it were proper in a case of this kind, to leave questions of interest out of view, every one feels that it concerns the honor of the country, that we should not continue to depend upon the Hydrography of foreign governments for those charts which will enable an American vessel to pass in safety through such a common thoroughfare of our navigation as the Gulf of Florida.

The very interesting and instructive exposition made by Prof. Bache, on the evening of the 16th, of the progress and extent of the work, renders it unnecessary for the committee to enlarge upon those points. They are convinced that all who listened to that exposition, will agree

with the committee, that the undertaking is in all respects of truly national magnitude and character. The great length of coast to be surveyed in reference to so many commercial and territorial centres, and the necessity of the utmost accuracy which can be attained in the present state of science, make the survey unavoidably a work of much time; and the large body of coöperators by land and water, in the field and in the bureau, possessed of talent, both scientific and practical, of the highest order, implies a heavy expenditure. While it follows as a necessary consequence, that a watchful economy should preside over the administration of the work, it is not less obvious that it cannot be safely hurried in point of time, and that it ought not to be stinted in reference to the number and qualifications of the persons employed. No scientific performance in this country will be brought into comparison so directly with similar undertakings abroad. None stands in more immediate connection with great national interests; none covers so extensive a field, or comprises operations so complicated and multifarious. Under these circumstances, it will not excite the surprise of any considerate persons, if a series of years and a large expenditure are absolutely necessary for its accomplishment.

The committee feel it their duty to express to the Association the strong conviction that the manner in which the survey has been conducted by the present superintendent and his associates, is in a very high degree creditable to those gentlemen and to American science. This opinion is formed not only on the highly satisfactory exposition of Dr. Bache referred to the committee, but upon the official reports of the survey published by order of Congress, on the surveys and charts which have been already engraved, containing a portion of the results hitherto attained, and on other documents, well known to the members of the Association, relative to the operations of the survey in various departments of science.

The committee earnestly hope that Congress will not be discouraged from the further prosecution of the survey, by the necessarily heavy expenditure required to carry it on; which, however, is believed to be materially less than the expenditure for similar purposes by several governments in Europe. The enterprise is now in the best hands, and in the most favorable train for successful prosecution. It has been the good fortune of the government to secure for the direction of this great work the services of a gentleman, whose eminent talent — whose early training in the National Military Academy — and whose acknowledged position among the men of science of the country,

signally point him out for the trust. A corps of assistants has been gradually formed in the various branches of the survey, whose skilled experience, if withdrawn by any suspension of the work, could not be replaced for years, nor wholly lost without great detriment to the public service. The committee are decidedly of opinion that an enlightened public sentiment will require the completion of the survey as projected and thus far successfully prosecuted by Prof. Bache, and they are consequently persuaded that a wise calculation of economy would dictate the continuance of those appropriations, which are needed for the prosecution of the work on the present scale of operations.

The Committee accordingly recommend the adoption of the subjoined resolution.

All of which is respectfully submitted by

EDWARD EVERETT,
BENJAMIN PEIRCE,
A. CASWELL, } *Committee.*

Cambridge, August 21, 1849.

Resolved, As the sense of the American Association for the advancement of Science, that the manner in which the Coast Survey of the United States has been conducted by Professor A. D. Bache, and those associated with him, is highly creditable to American Science; that it promises great benefit to the Commerce and Navigation of the country; and it is eminently entitled to the continued favor of Congress.

Lieut. MAURY said he concurred very cordially in that report, so far as it goes; but he thought it did not go quite far enough. He thought the coast survey was too limited, and that it ought to embrace the *currents*, as well as the *rocks* and *shoals*. He believed there was a westwardly current, which, if ascertained, would shorten the passage from Havana to our Gulf ports probably one third.

Pres. EVERETT said that, in the enumeration of the objects of a survey, in the early part of the report, the *currents* were mentioned as one of those objects. And as the subject of currents was in the hands of another committee, he thought it might be left to them.

Lieut. MAURY was under the impression — and the superintendent of the coast survey would correct him if he was wrong — that the law confined the survey within a certain distance from the coast, or within

a certain depth of water. He thought the proper survey could not be made without an alteration of the law.

Prof. A. D. Bache said there was no limitation of the law that applied to this case.

The report and resolution were adopted.

The following resolutions were then offered, and severally agreed to.

The following was offered by Pres't. Everett :

Resolved unanimously, That the thanks of the American Association for the Advancement of Science be presented to Professor Joseph Henry, their President, for the courteous, dignified, and highly acceptable manner in which, during the present meeting of the Association, he has performed the duties of the Chair.

Prof. Henry responded, and thanked the members of the Association for their support, without which he should not have been able to discharge the arduous duties devolved upon him.

The following was offered by Prof. Bache :

Resolved, That the thanks of this Association be returned to the Corporation of Harvard University, for the very liberal provision made by them for the accommodation of the members and meetings of the Association.

The following was offered by Prof. Hall :

Resolved, That the thanks of the American Association be presented to the Ladies and Gentlemen of Cambridge, for contributing in so many ways to our individual and social comfort and pleasure.

The following was offered by Prof. Agassiz :

Resolved, That the thanks of the Association be tendered to J. I. Bowditch, Esq., of Boston, for his invitation to a dredging excursion in Boston harbor, in the steamer R. B. Forbes, on the 22d inst.

The following was offered by Prof. Peirce :

Resolved, That the thanks of the Association be given to Mr. U. A. Boyden, for his invitation to visit Lowell and Lawrence on Thursday, the 23d inst.

The following was offered by Prof. Lovering :

Resolved, That the thanks of the American Scientific Association be presented to Prof. Horsford, for his able and courteous discharge of the duties of the Secretary of the Association.

The papers for the several Sections were then announced, and the Association adjourned to 4½ P. M.

E. N. HORSFORD, *Secretary*.

Seventh Day, August 21, 1849.

SECTION OF MATHEMATICS AND ASTRONOMY.

LIEUT. CHARLES H. DAVIS was called to the chair.

The first paper was an oral communication

ON THE GAUSSIAN TABLES. BY DR. B. A. GOULD, JR.

THE name of this paper was entered upon the list because, having devoted some time to the study of these tables, and having computed them to ten decimal places with a view to their publication in a form more convenient than that of Matthiessen, I was desirous of mentioning some of my own labors respecting them, and of calling attention to some of their beautiful properties not generally known. There are two tables which, as their illustrious author has said, are complementary to one another. The one is the table called Logarithms for Addition and Subtraction, and the second gives, for the argument log. x, the logarithm of $\frac{x+1}{x-1}$.

By the former, the Gaussian Tables commonly so called, the argument or column A. being log. x, we have in the two columns B and C the logarithms of $1+\frac{1}{x}$ and $1+x$. Two numbers being given by means of their logarithms, we can by these tables at once obtain the logarithm of their sum or difference without taking out the numbers themselves. But quite as elegant properties of these tables are to be found in their adaptation to the solution of numerical equations by approximation. Prof. Gauss pointed out some of these, for the first time, in the second edition of his Tables in Vega's *Handbuch*. There are many others which he did not then mention. To some of these it was my privilege to have my attention called by Mr. Gauss, others I have myself developed. The second table was computed to five

decimal places by Weidenbach, and printed as a supplement to Schumacher's Journal in 1829. This I have also calculated to ten decimals. But it is the last day of our session, and I pass over the tables with this allusion, in order to be able to say something upon a subject which has, at least to-day, a stronger claim to our attention.

Prof. PEIRCE remarked that there is a method of using the common tables for the purpose of adding and subtracting, in which it is necessary to open the tables but once for the sum, and once for the difference.

[Prof. P. then explained this mode of using the tables.]

He then remarked upon the necessity of recomputation of the Gaussian Tables, as of any mathematical tables, in order to render them reliable. It wonld be long before the mistakes could be discovered in any other way.

Dr. B. A. GOULD said that the method used by Prof. Peirce, was analogous to a computation of the numbers which would have been given by the Gaussian Tables, the principle used being an elegant modification of that upon which they were founded. He also stated that he had just learned from Schumacher's Journal that an edition of these tables had just been published, by Dr. Zech. The latter has arranged his tables in three columns, the first containing the argument, the second the numbers to be used in addition, and the third the numbers to be used in subtraction; this deprives the table of its elegant properties for the solution of numerical equations by approximation.

ON KIRKWOOD'S ANALOGY. BY DR. B. A. GOULD, JR.

THE subject which Mr. Walker brought to the notice of this Section on Saturday, is one of far more than ordinary interest. Besides the elegant simplicity of Mr. Kirkwood's formula, his theory must, if it be confirmed, materially influence our views of cosmogony and of the theory of the Universe. I have devoted all of the time which my duties have allowed since Mr. Walker made his communication, to the numerical examination of the Analogy to which he referred and which prompted his beautiful investigations. I will state the results, though not in the fulness which they deserve, and with which I at first hoped to be able to give them; for although the subject is large, and one which we cannot expect to exhaust for many years, yet the time

of the Section is so precious at this late hour, that I shall limit myself to as brief a statement as possible.

Mr. Kirkwood's theory, as regards the rotation of the planets will, if found to be true, — and the presumption seems to-day strongly in favor of its truth — furnish a remarkable and unexpected argument in support of the Nebular Hypothesis. The minds of many have been wavering of late with regard to this hypothesis; their doubts have been strengthened by the unqualified assertions that all nebulas are resolvable; but this analogy of Kirkwood tends most strikingly to confirm it — so much, indeed, that if this latter be true, I do not know how any one can resist the argument which it furnishes in favor of the former, in so far as it applies to our solar system. It is then no longer a hypothesis, but becomes a probable theory.

I will give a very short sketch of the quantities I have used in repeating Mr. Walker's computation.

[Dr. Gould then gave upon the blackboard the masses of the planets, and the periods of rotation which he had used, differing from those used by Mr. Walker.]

These are the masses which I have used; and these are the times of rotation as given in the books. I do not know how accurate the latter may be considered; perhaps to minutes, perhaps even less so. I believe they have all been determined by the observation of spots. If so, what proof have we that the spots do not move, no matter what the number of rotations used in determining the period? I have marked the period of Uranus as doubtful, because I do not know upon what authority it rests, having only found it in a table of a popular work by Sir J. Herschel, with a mark of doubt prefixed. It does not agree at all with this theory.

In considering a question of this kind, we must remember the nature of our investigations. The subject is to a certain extent, necessarily general, and the appearance of precise harmony could not be expected even were our data exact, which they are not. The nature of the problem requires a general, not a special agreement between observation and theory. When we are considering the evolution of order from chaos, we cannot pretend to a knowledge of all the physical forces which exerted an influence. We go back to a supposed time when the planetary spaces were filled with nebular matter; we assume the existence of certain nuclei or centres of attraction; and, from our knowledge of the solar system, as it now is, infer the relative force which these several centres of attraction must have exerted,

and assign to each its proportionate realm. If now we find that the spheres of influence belonging to the several nuclei are harmoniously connected, by a simple formula, with the periods of rotation as observed to-day, — an element before omitted in our investigations — we discover a remarkable corroboration of the probability of our hypothesis. This is what Kirkwood's formula professes to be — a simple relation between the time of rotation and the diameter of the sphere of attraction.

The subject being then a strictly general one, we are not warranted in demanding that exactness of numerical agreement, requisite for the verification of theories of a more special nature. If, as circumstances appear to indicate, more careful investigation should lead to the general adoption of the theory of Kirkwood, I should not desire that this should be denominated a LAW. Nature's laws must be precise and complete. The relation which we are considering claims only to be approximate — an analogy. And in speaking of it, I shall call it by this name, *Kirkwood's Analogy*. And where we have by hypothesis a right to expect analogy and not perfect accordance, the want of perfect accordance must not be considered to cast doubt upon the theory. Besides, if Kirkwood's Analogy were the result of a general law, would not the action of the law be modified in all probability by circumstances which would prevent us from perceiving any strict mathematical precision? The considerations which would be strong arguments against hypotheses of other kinds, do not appear to me weighty when applied to any thing so rude as the motion of chaotic matter.

There is a formula known as "Bode's law" — an empirical formula — expressing a supposed analogy, for which no reason was ever assigned and which, even before it was broken by the new planet Neptune, was found utterly devoid of that universality and precision which must characterize all laws of nature. Though it was considered a remarkable coincidence, and perhaps as capable of suggesting some law of nature, no true mathematician could ever have regarded it as a real LAW. Moreover, Gauss had shown long since that it did not hold for Mercury.

This "Law of Bode" was analogous to the theory of Kepler, that as there were but five regular solids, there could be but five planetary intervals, and therefore no planet between Jupiter and Mars.

Kepler's theory was totally overthrown by the discovery of Uranus, as the other has been by the discovery of Neptune. Bode's law, to

which, by the way, Bode's name has been improperly given, would make the distance of Neptune beyond the orbit of Uranus nineteen times the distance of the Earth from the Sun, while it is in fact less than eleven times this distance beyond it, so that the fallacy of this formula must now be so evident as to require no demonstration.

Discordances such as those which exist in the application of this law to the planetary system, would afford sufficient reason for rejecting the analogy of Kirkwood; but, with even these discordances, the fact, that a single formula would approximately represent the truth to so great an extent, would justify us in bestowing much time upon its consideration.

[Dr. Gould then gave a brief sketch of the points of connection between the nebular hypothesis and the new analogy,—showing how the one would lead to the other.]

It will be remarked that in the phrase "sphere of attraction," the word sphere is not used in its geometrical sense. Nor is a planet necessarily in the centre of its sphere of attraction, for upon the one side, as is the case with the earth, may be a planet comparatively near, and upon the other a smaller planet at a greater distance; whence it is evident that the extent of the sphere of attraction will be much less upon the former side than upon the latter.

In Mr. Walker's theory he assumes x, in the equation

$\theta = \frac{2\pi}{x} \cdot a^{\frac{3}{2}}$, to be a constant and equal to $\frac{1}{(k)^{\frac{3}{2}}} \cdot \frac{1}{365.256374}$, the term k being nearly 2. In the following formulas I shall denote the quantities which refer to the Earth by a single accent, those referring to Mars by two, to Jupiter by four, and to Saturn by five, reserving three accents for the hypothetical planet between Mars and Jupiter. I make use of Mr. Walker's formula for the sphere of attraction as follows:—D being the diameter of the sphere, a being the mean distance of the planet, and m being its mass.

For Jupiter we have

$$D^{\text{iv}} = \sqrt{m^{\text{iv}}} \left(\frac{a^{\text{iv}} - a'''}{\sqrt{m^{\text{iv}}} + \sqrt{m'''}} + \frac{a^{\text{v}} - a^{\text{iv}}}{\sqrt{m^{\text{iv}}} + \sqrt{m^{\text{v}}}} \right)$$

So for Mars;

$$D'' = \sqrt{m''} \left(\frac{a'' - a'}{\sqrt{m''} - \sqrt{m'}} + \frac{a''' - a''}{\sqrt{m''} + \sqrt{m'''}} \right)$$

In these formulas you will perceive that every thing is known, except the mass and distance of the new planet; or the *old* planet, if you please. The only assumption is the truth of the nebular hypoth-

esis. If we knew the values of D^{iv} and D'', the spheres of attraction of Jupiter and Mars, we should have two equations and but two unknown quantities, a''' and m''', and should thus have this planet restored by the nebular hypothesis alone.

I have assumed in the computation for the value of k, not 2,— the constant, which Mr. Walker supposes it to be, not only from his calculations, but from *a priori* reasoning,—but the mean of the values obtained from each planet, using the masses as given in the books, though affected of course, with some inaccuracy.

Now recurring to the above phenomena, let us take, for the sake of convenience,

$$D^{iv} - \left(\frac{a^{v} - a^{iv}}{\sqrt{m^{iv}} + \sqrt{m^{v}}}\right)\sqrt{m^{iv}} = A,$$

$$D'' - \left(\frac{a'' - a'}{\sqrt{m''} + \sqrt{m'}}\right)\sqrt{m''} = B,$$

and then we have,

$$\frac{m'''}{m^{iv}} A = a^{iv} - a''' - A,$$

$$\text{and hence } \sqrt{m'''} = \frac{a^{iv} - a'' - (A + B)}{\frac{A}{\sqrt{m^{iv}}} + \frac{B}{\sqrt{m''}}}$$

Here all the quantities on one side are known, and we use the mass of the new planet thus obtained for the solution of the problem. This being substituted in either of the previous equations, will give us the mean distance of the planet.

Or, if for convenience we make $\frac{A}{B}\sqrt{\frac{m'''}{m^{iv}}} = C$

we shall have— $$a'' = \frac{a^{iv} - A + (a'' + B)\,C}{1 + C}$$

Now the only question is as to the value to be adopted for Mr. Walker's constant, which it seems to me should be deduced from observation only.

In computing the value of k, I obtain for

Venus, ...	1.9374
Earth, ...	1.9030
Saturn, ...	1.9763
The mean of this, ...	1.939

is the quantity assumed for k, in obtaining the results of which I shall speak; these three planets being the only ones to which the formula

can be applied. It will be seen that the values obtained are most confirmatory of Mr. Walker's results. Unless we suppose the nebular matter to have been equally distributed through the solar system, we could not expect to find k absolutely *constant*, even if it were an approximation to the number 2.

Prof. WALKER here remarked that the constant used by Dr. Gould answered better than the constant 2.

Dr. GOULD. We do not know the extent of Mercury's influence inside its orbit, and hence cannot know the diameter of its sphere of attraction. Nor can we apply the formula to Mars or to Jupiter, for we do not know what planet may have been between them. We cannot apply it to Uranus, for we do not know its period of rotation. There remain but the three planets mentioned above.

Calculating, from the equations thus developed, the mass and distance which would belong to a planet between Mars and Jupiter, and thence, by Kirkwood's analogy, the corresponding time of rotation, we do not find it so great, that, by mere centrifugal force, the planet could have been exploded, and its mass scattered in the form of asteroids.

From a very rough computation of the place and size of the hypothetical planet, I obtain a mean distance 3.12,—and a mass $\frac{1}{501,500}$. This mass is very much smaller than the mass of our earth, and would agree with the supposition of a small planet, smaller even than Venus, but would be at least equal in size to twelve or fifteen Asteroids.

This gives rise to a great many speculations, most interesting and important in their bearing upon the theory of the universe. I wish to dwell upon the fact that we neither know accurately the period of rotation, or the mass of most of the planets. The only element which is really well known, is the distance of the primary planets from the sun. Then there is the difficulty to which I also alluded, in ascertaining the magnitude of spheres of attraction, that we cannot assume the nebulous matter to be equally dense; so that it cannot be demanded that the analogy should be very accurately expressed by any given data.

It is now extremely important that observations should be made upon the periods of the rotation of several planetary bodies, and is much to be desired, as bearing upon this problem, that those who occupy themselves with what may be called the natural history of astronomy should determine the times of rotation anew, and thus

enable us to decide upon the truth of a law, the discovery of which may be important in the history of Astronomy.

Prof. Walker made a remark on Saturday, with reference to the position to which Mr. Kirkwood will be entitled, should his theory be found true. The Section seemed surprised at this remark. I do not wish to express myself strongly, but certainly when we look back upon the labors of Kepler, who strove so many years with results so unpromising, until he discovered the laws which underlie the whole fabric of our solar system, and then turn to Mr. Kirkwood, a teacher in the interior of Pennsylvania — who without the sympathies of kindred minds, or the use of any library of magnitude — without calling even upon the aid of strict mathematical analysis — has fixed his attention upon this one problem, and investigated it in all its bearings, until after ten years of patient thought and labor, he has arrived at such a result as this — we cannot but be struck with the similarity of the two cases; nor can we consider it as very derogatory to the former to speak hereafter of Kepler and Kirkwood together as the discoverers of great planetary harmonies.

Prof. Walker followed with some remarks upon the same subject. He gave the formulas of the nebular theory upon the supposition that the substance is homogeneous, &c., and of rotation, as connected with Kirkwood's analogy. From these calculations, he derived results relative to the mode of formation of the earth from the ring, the form in which it must have existed according to that theory.

On Geometrical Interpretation of Analytical Notation. By J. Patterson.

This long communication was of such nature that no abstract could be made.

On a Curious Phenomenon Relating to Vision. By Prof. Lovering.

Plateau, Wheatstone, and Faraday have made ingenious researches on the appearances exhibited by wheels in rotation. The optical deceptions which they describe all turn upon that organic peculiarity of the eye called the persistency of impressions on the retina. The phenomenon to which this paper relates also originates in an organic law of vision, but one wholly distinct from that just mentioned.

The phenomenon is as follows. When the plane of rotation of an ordinary windmill nearly coincides with the axis of vision of an eye which is looking towards it, we have observed that the mill will *appear* suddenly to shift to the other side of the axis of vision, and turn in the opposite direction. In a few minutes it will resume its first position, and then change again; and so on repeatedly as long as we continue to look steadily at it. The change in the direction of rotation is always associated with the displacement of the plane of rotation. The observer may soon satisfy himself, from the direction of the wind and otherwise, that both these changes are apparent and not real. Of course, therefore, the wheel is not seen to move from one side of the axis of vision to the other; it does not move at all. The change originates in the eye and in the judgment which the mind passes on the facts of sensation, and is not subject to the law of continuity.

FIGURE 1.

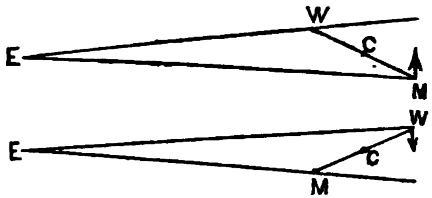


FIGURE 2.

Let W M represent the intersection of the place of rotation of the windmill with the horizon; let E be the place of the eye, and E W the direction of the extremity of the left horizontal arm of the mill, and E M the direction of the end of the right horizontal arm. What we wish to explain is, why the line W M, which is really in the position assigned to it in figure 1, appears sometimes to occupy the position which we have given it in figure 2.

We start with what we think must be admitted to be a general fundamental law of vision, namely, that the eye is so organized that it is able directly to see the direction of a point, but not its distance in a straight line from itself. The eye can see directly the distance of two bodies from each other, at least that resolved portion of it which is perpendicular to the *mean* axis of vision, because this portion subtends an angle which represents the difference between two directions. The measurement of such a distance is embraced in the provision for seeing direction. All these methods used in Geodesy and Astronomy to obtain that element of the distance which is parallel to the axis of

vision (by parallax, as it is called,) depend on the geometrical relation existing between this unknown distance and certain directions which are seen and determined at once by the eye. We take the direction of a body from two stations; those angles, with the distance between the stations, furnish all the requisite data for the solution of the problem. We cannot, in such a case, be said to see the distance of the body, although all the data are obtained by sight. For the directions are not observed together, but in succession; or, if at the same moment by two different observers, we can hardly call that the result of direct sensation which requires two persons. Moreover, in either case, the calculation which gives us the distance may follow at a long interval.

These angles which, in important cases, are measured by nicely graduated instruments, accurately pointed by the help of lenses, are ordinarily judged of, or guessed at, by the eye. And these calculations which, for the delicate objects of science, are laboriously and leisurely performed with tables of logarithms, may be instantaneously and unconsciously gone over in the mind. In such cases, we think we *see* approximately the distance of a body. But the nature of the process is the same; it is the same mind which geometrizes, though the data and calculations, and consequently the results, do not aspire to an equal degree of accuracy. As this process takes so little time, as the observer may not move his body, or even his eye, he easily persuades himself that he enjoys the power of seeing distances. Its failure in many exigencies best reveals the character and extent of this power.

1. In the first place the two eyes, directed from their two positions in the head, upon the same body, as two telescopes upon two remote mountains point to the signal that shines upon a third, furnish the data for calculating the distance of the body. A single eye, if the head move, can do the same work; it may do it much better if we move the head further than the interval between the eyes, and, best of all, if we move the whole body and enlarge our base of operations. Whenever the distance of the object is very large, compared with the interval between the eyes or the base line, we have what is called an ill-conditioned triangle, and our unconscious geodesy fails us; we commit great errors in estimating the distance, or are wholly at a loss about it.

2. Another method, and which can be practised by a single eye, rests upon the adaptation of the focus of the eye to different distances. By the effort made to change this focus, we are informed of the diver-

gency of the rays which have entered the pupil, and consequently the distance of their radiating centre. It would not be difficult to trace an analogy between this operation and that which enables us to measure an air line across seas and mountains. But in this miniature exhibition of the process, the base line is no larger than the diameter of the pupil, and we arrive very soon upon ill-conditioned triangles. As the distance increases, this second method fails us much sooner than the first method. Whenever the distance of a body is large, compared with the focal length of the eye, the rays from it are sensibly parallel and remain so for all larger distances. The rays, therefore, from all bodies outside of this limit have sensibly the same focus, and consequently their exact distances cannot be appreciated through the change in the focus of the eye as it passes from one to the other.

Assuming now that a body is within such limits of distance from the eye that its distance may be approximately calculated from such data as the eye itself will furnish, by one or other of the two methods described, it may still be true, if the body is small compared with its distance, that the observer will not be able to say *which parts* of the body are *nearest*, and which are *most remote;* and now we come to the case of the windmill. If the arm on the left is the nearest, then the plane of rotation is as is represented in figure 1. If the arm on the right is the nearest, the position of this same plane is as it appears in figure 2. So far as this position is to be settled by an accurate comparison of the relative distances of the several parts of the mill, it is obvious that our judgment must be at fault, whenever the wheel is at any considerble distance, for want of precise data on which to ground its calculations. The only thing which the eye can see accurately is the direction of the different parts. If the distances are eliminated, the position in figure 2 represents the directions as faithfully as that in figure 1. Whenever we know beforehand the real shape of a body, we have some assistance in ascertaining its real position. Suppose we know that W M is a straight line turning upon its middle point; if the position is as in figure 1, W C must subtend a larger angle than M C. The contrary will be true in the position of figure 2. Let us now call up the image of the whole mill; let us suppose that we know it to be a circle presented obliquely to the eye; our geometrical sense will tell us, that if the left side is turned away, the centre of the wheel will be projected on the ellipse of projection of the whole wheel nearest to the left edge. If the right side is most remote, the projected centre will be towards the right edge. An eye,

nicely disciplined in perspective, has here the means of determining the position of the plane from the relative fore-shortening of its parts. Suppose the observer to use both eyes, that comparison of the images which gives the position of the body will be still easier. For the images of the wheel on the two eyes will be different; and the image which is on the left eye, when the wheel has the position in figure 1, will be on the right eye, when the wheel is in the position of figure 2. Finally, we may invoke the aid of the lights and shadows to confirm us in our decision. If the sun, or principal source of light, is on the right, then the side at which we look will be illuminated when the real position of the mill is as in figure 1; but it will lie in shade when the position of figure 2 is the true one.

If this view of the subject be correct, we certainly cannot be said to see directly the position of a body by a simple effort of sight. By means of this organ we gather up a variety of data, which are submitted to a rapid mental analysis. When the body is near, the differential qualities on which our judgment is based are substantial and obvious, so that he who runs may read. As the distance increases, the calculation grows nicer; only a careful, scrutinizing eye will notice the delicate touches in outline, proportion, or shading, on which the problem hangs. At still greater distances it is impossible for any eye, or any mind, however geometrical, to divine the true position. Before this limit is reached, our decision is held by so weak a tenure, that a slight circumstance, the absence of strong sunlight, an obliquity in the eye, or a scarcely perceptible motion of the head, is sufficient to alter it. The mind halts between the two opinions; at one moment thinking the body is in one of the possible positions, and the next moment believing as confidently that it is in the other.

This instability in the judgment which reason pronounces on the testimony of the eye, in doubtful cases, is not of rare occurrence. Many have been struck with the difficulty of determining in which of two positions a flag or a vane is situated; and, therefore, in which of two widely different directions the wind is blowing. Of two observers at the same spot, one may affirm that it is in one position, while the other is equally sure that it is in the other position. The deception in regard to the direction of rotation, though a necessary consequence of the displacement in position, has not been so frequently observed, and has never, so far as I know, been explained. Suppose a wheel, whose plane of rotation is in a vertical plane nearly coincident with the axis of vision. What distinguishes a rotation in one direction

from a rotation in the opposite direction, except that, if, in one case, the edge nearest the eye is descending, in the other case it will be ascending? But the displacement which has been under discussion consists in an illusion as to the relative distances of the parts; those which are nearest being placed as if they were the most remote. If, therefore, in the true position, the nearest parts are ascending, in the fictitious position, the remote parts will be ascending, and hence the motion will appear as one in the opposite direction to the real one.

Prof. SNELL remarked that he had often noticed similar optical illusions. He thought that an uncertainty between two such positions of the object, as would give the same projected figure, usually occurred, where there was no decided light or shade falling upon it, to aid the observer. But on the other hand, if the observer was in any way deceived with regard to the direction in which the light came, instead of being in doubt, he would make a wrong judgment as to the position of the object or the nature of its surface. He then instanced the illusion in an inverting microscope, where the direction of the light being known, a raised surface is liable to appear depressed, and vice versa.

Prof. HENRY stated that he had noticed the same illusion just before leaving home. Dr. Foreman and himself, endeavoring to ascertain the direction of the wind, referred for this purpose to the motion of a windmill near by; they disagreed as to the direction of its rotation. The phenomenon struck him as singular, but his mind at the time was much occupied with other subjects, and he did not attempt to make out the cause.

There is another illusion belonging to a similar class; I refer, said Prof. H., to the apparent motion of a body at rest, and the converse; this is an illusion of very frequent occurrence, but which I have not seen as minutely analyzed as might be desirable. I was led, a few years ago, to reflect upon the cause of it from this circumstance. Standing with two of my little girls, upon the edge of the canal basin, and looking down upon a boat gradually approaching the place on which we stood, I observed that suddenly the motion of the boat was apparently transferred to the wharf, and at the same instant each of the children exclaimed, Oh! I am moving. The question immediately occurred to my mind, why should these observers be subjected to this illusion at the same moment? What are the conditions necessary to produce this effect? The following hypothesis suggested itself as

the most plausible answer, namely : By long experience, the mind has acquired the habit of inferring without reflection, that when two bodies are relatively in motion, the real motion belongs to the smaller, and that the larger is at rest. It is not necessary to the effect, that one of the bodies should really be the larger, but that at the time it should occupy the larger portion of the field of view. Thus in the case of the boat; while it was at a distance, and occupied but a small part of the field of view, it was the smaller object, and with reference to the wharf appeared, as it was, in motion; but, when it came almost directly beneath the observers, so as to occupy the principal part of the field of view, and consequently the greater share of attention, it became the fixed body, and the wharf the moving one. To bring this hypothesis to the test, an experiment was instituted on the spot; a new position was taken in advance of the boat, and as it approached this, the same phenomenon was again exhibited; the wharf and the observer began to move; the eye was then gradually turned, so that the boat would occupy the smaller portion of the field of view, and the wharf became the larger object. The result was as I had anticipated. At a given position the motion of the latter instantly ceased.

It was observed, however, that there was a tendency in the mind to persist in its affirmation, if it may be so called, as to the motion. When the wharf once appeared to get into motion, the illusion did not vanish immediately when the boat became apparently the smaller body, but it required that the wharf should be considerably larger, before the effect was produced. In the same way, when the mind had concluded that the boat was the moving body, it required an interruption of the attention, together with a much greater apparent size of the boat, to transfer the motion to the wharf.

The illusion is most perfect, when the moving body occupies the whole space around the observer. Visiting, said Prof. H., a few months ago, the Observatory of my friend Mr. Hallowell, of Alexandria, the whole external covering of which, namely, the cylindrical sides and hemispherical dome was made to revolve, while I was within. The illusion in this case was so perfect, that without thought, I exclaimed "stop! I shall fall, and the instruments will be all deranged."

The same apparent transfer of motion may be produced in another way, depending however, on the same mental tendency. If we are in a car which is in a state of rest in a depot, after we have travelled

several hours on the railway, and another car starts from along-side, the motion scarcely ever fails to be transferred in appearance to the car in which we are placed. In this case the mind has been assured by the previous experience of several hours, that the car in which we have travelled, and not the external objects on which we have looked, is in motion, and without reflection the conclusion is formed, that this is still the case.

The same tendency of the mind, was probably concerned in causing the apparent difference of direction in the rotation of the windmill, as observed by Dr. Foreman, and myself. All the physical conditions, as described by Prof. Lovering, were in this case probably the same to his eyes and to mine. If, however, one of us had frequently seen the mill turning in a particular direction, the preponderance of prejudice would be given in his mind to that direction, and thus determine the apparent motion.

All phenomena of this kind are connected with the instinctive generalizing tendency or law of the mind, which leads us to believe that what has happened, will happen, and that under like, or apparently like conditions, like effects are produced, a tendency which is the source of all our general truths, and perhaps of most of our popular errors.

Prof. JOHNSON cited the deception which, no doubt, very many persons had experienced, when, in a railroad car, and at rest, the motion of a car or train of cars, on an adjacent track, is transferred, in the conception of the observer, to the car in which he is seated. This ocular deception, is particularly liable to occur at depots and other stopping places, after a temporary stoppage, and while, apparently, the sensorium is more or less vividly impressed with the effect of passing by objects at rest. The mind having been for some time continuously affected by the observer's own motion, while going by stationary objects, he is led involuntarily to attribute the observed motion of other objects across his line of vision to the same cause, and if the train be passing him in a direction opposite to that in which he has been travelling, and be a long one, the uncertainty felt, becomes heightened and sometimes almost painful, so that the traveller is fain to relieve himself from it, by looking out to see whether in reality the wheels of the contiguous train are revolving or not, or to look to the opposite side of the train in which he is seated. On badly constructed roads and in rapid motions, other senses come in to correct that of sight. According to the experience of Prof. J. we are not so apt to refer the motion of our own car to another at rest, as that of

another in motion to our own when at rest, and this perhaps for the reason that our impressions have been derived from our own motion, while passing by stationary objects. Ocular spectre and their duration on the retina are familiar facts. But in the rapid succession of objects in the case of railroad travelling, the only impression in any degree durable is mere apparent motion, in objects near us, and this appears to heighten the illusion arising from standing on shore, and seeing a large body near us in motion.

Lieut. MAURY mentioned the fact, that at sea, the other vessel always seemed to be flying while they were apparently motionless. It made no difference as to the relative size of the two vessels; one might be a seventy-four, and the other a fishing-smack. He ascribed it in part to the fact that the muscular activity which normally produces motion, was not in exercise.

Dr. GOULD remarked that the explanation of Prof. Henry would cover this case, for the vessel on which the observer is situated would occupy the larger portion of the field of attention.

Prof. PEIRCE mentioned a case where he met a train of cars in motion, and found it impossible to walk straight, while his eyes were fixed upon it, on account of the transfer of motion. It had been impossible to ascertain, for the same reasons as apply to the windmill, whether the satellite of Neptune moved in a retrograde or direct obit.

Prof. HENRY remarked that these instances showed how necessary it was to learn to see. The senses never deceive; the error is always in the mind. The mind draws wrong conclusions from the evidence of the senses. He mentioned an instance where he had been seasick, and afterwards supposed the room to be in motion, and himself to be on shipboard, merely from the undulation of the carpet, by the wind.

Dr. HARE mentioned the deception in the sense of feeling, arising from crossing the fingers and placing a ball between, when it will appear double, on account of our habits being to refer two such sensations to opposite points.

Prof. HOLTON remarked upon the illusion produced by looking at the track from the end of a train of cars, when going at a rapid rate. Every bush, rock, and bridge seems to be contracting its dimensions very rapidly — crawling into itself, as it were. By continued observation, the eye would become accustomed to the motion, and the effect would not be produced.

Lieut. DAVIS mentioned as an illustration of Prof. Henry's theory, the apparent motion of the moon through large masses of clouds, and the motion of small clouds by the moon.

ON THE ESTABLISHMENT OF AN ASTRONOMICAL JOURNAL IN THE UNITED STATES. BY PROF. J. S. HUBBARD.

I PROPOSE saying a few words on the establishment of an Astronomical Journal in this country. That such a journal, once established, would be of great benefit, in promoting research and in diffusing results, none can deny ; the only question is, therefore, is it expedient *now* to commence the work ? The proposal is at the outset met by difficulties apparently neither few nor small. Persuaded, however, that upon closer consideration these objections will lose somewhat of their force, we venture to examine them a little more in detail, and then perhaps we shall be able to consider with less misgiving the advantages to be derived from such an enterprise.

But before proceeding to these considerations, it would be well to show what we mean by an Astronomical Journal. It is not to be what is called a *popular* work ; that is, its object is not to reduce scientific results to ordinary language, for the purpose of rendering them intelligible to the mass of unscientific readers. Nor is it to be the organ of the pseudo-scientific, who may attempt to build up a reputation upon words. But the field which it offers should be open to all who in earnestness and in truth desire to coöperate in enlarging the domain of astronomy. The student who desires to learn what others are doing, and in what manner results are obtained, and the man of established reputation who may wish to communicate to others the light that has dawned upon his own mind, may here find a ready means of intercourse with others.

Now let us look at the objections urged against the *present* establishment of such a journal. It is said that the paper will not support itself. The force of this we can hardly admit. It is proposed to publish it, not at stated periods, but irregularly, like the *Astronomische Nachrichten* — to publish at least one number every month and oftener if convenient. A volume will consist of a definite number of pages, and consequently will embrace a longer or shorter period of time as the case may be. This will be a matter of both necessity and convenience. Now it is true that the receipts of the first year will not bal-

ance the expenditures, it may be that the second year will still show a deficit; but we are provided against even more than this. There are those who have pledged themselves to make up for three years or even more — any amount that may be necessary to sustain the journal. This pledge has been given after a careful estimate of the expense of the work. If after a trial of three, or even two years, other objections more forcible than this should continue to exist; if the importance of the journal and its effect upon the science of the country have not become sufficiently evident to be its own best argument for support, we would then be content to say no more, and to relinquish entirely the further prosecution of the work. Such a journal ought not to support itself. We do not dislike to see this objection urged. It implies the conviction on the part of those who present it, that what we have already said concerning the character of the journal itself, should be adopted as true. Its purely scientific character excludes from its columns much that, not bearing directly upon the progress of astronomy, would still be of interest to the general reader, and it limits its resources entirely to those contributors whose object is only the promotion of astronomical science. The readers and the contributors will therefore at first be few; and if, as we just now remarked, at the end of three years, during which this objection must remain invalid, its force be then acknowledged, it were better that the journal should cease. But we are persuaded better things of it, and are willing to see the risk tried. The want of an editor, competent and at the same time willing to undertake this labor, has been, until recently, an objection. We are happy to say that one of our own number, a member of this Association, known not only among ourselves, but abroad, and with whom we believe the astronomers of this country will unite in cordial coöperation, has expressed himself willing and ready to undertake the task of editing the Journal.

The strongest argument brought against us is the lack of material, of communications such as we have alluded to, sufficient to fill the columns of a periodical Journal. This is a matter of conviction, it may be of partial experience, and we cannot gainsay its force. But there are several things to be thought of, in connection with this. We have two classes of matter to look for — observations and investigations. Of the former, there is already a store in hand, never published. Observations are increasing among us, and scarcely a clear night passes but something of importance is determined, some fact noted which but for our astronomical Journal, might remain forever in the pages of the observing-book.

Much more, too, may be fairly expected, as soon as the Journal shall have commenced its operations. The "esprit du corps" will stimulate many a one who sees more immediately than he can now see, the progress and the nature of observation; he will better learn what to observe and how to observe it. This, however, is yet to come. In the department of investigation where we hope to record the results of mental application, we have reason to expect much. There are those among us, and we may well be proud of them, who have become familiar with the mysteries, who can trace the operation of Nature's laws far beyond the point where many falter, and it is to these that we look for countenance and support. May we not hope that they, in their desire to communicate new truths and to enlighten others, will coöperate with us as we undertake the work of this Journal? We are assured they will. And moreover, we are to remember that our plan will include not merely astronomical observations and the results of mental labor in the department of physical astronomy. Whatever tends to increase our knowledge of the figure or magnitude of the earth — investigations in geodesy, meteorology, and kindred branches — will come within the scope of the Journal.

But let us suppose the paper established, and see if we can foretell any of the consequences. One thing is certain; it will give an impulse to the science. Emulation will be excited; an emulation that must be productive of some good result. The rapid diffusion of information will tend to awaken and keep alive a stronger interest in the progress of astronomy, and we doubt not that from many a source now unknown to us, matter of interest will come in, that many a one who is now debarred from the knowledge of what is being done in this department of science will rejoice thus to learn, and may perhaps contribute his mite to the furtherance of our plan.

But there is another inducement for us *now* to commence this work. As the astronomical science of our country becomes more and more developed, and assumes, as perhaps it will, a national character, the usefulness of the Journal we contemplate will become more and more apparent. It will be the embodiment as it were of American Astronomy. We wish it to contain in its successive volumes, the history of the astronomy of our country, so that every thing of any importance in this department which transpires from the commencement of the work, shall find here its place, either as given in detail, or by some allusion respecting it.

And what better time than the present shall we have for commenc-

ing such a work? Shall we wait till the history of the science — and we are already behindhand — shall become more developed? Much that is now past is to be recalled; shall we wait till we can no longer trace the beginnings of the history? And on the other hand, shall we wait for the accumulation of matter, till we may start full-fledged into existence? But matter will never accumulate and remain unpublished; as it now increases, it will flow through various channels, and he who would study the history of astronomy among us, though as yet the task would be by no means impossible, would at length become more and more perplexed, and would lose one great advantage which our present plan would afford him.

We must not wait for matter to crowd upon us; we ought now to commence, and we are willing to labor hard and abide by the result.

Under these circumstances, I have thought it advisable to present the subject in this manner before those who take an interest in whatever relates to the progress of science among us. I have hastily thrown together these thoughts, and feeling the need of the countenance and coöperation of others, have ventured thus to urge the subject before them, and to leave it with them to say whether we shall go on or not.

On motion of Prof. PEIRCE, referred to the following select Committee: —

Dr. B. A. GOULD, JR., Prof. J. S. HUBBARD, Prof. JOHN H. C. COFFIN, SEARS C. WALKER, Esq., Prof. JOSEPH HENRY, Prof. A. D. BACHE, Lieut. M. F. MAURY, Lieut. C. H. DAVIS, Prof. BENJAMIN PEIRCE.

ON AN AMERICAN PRIME MERIDIAN. BY PROF. I. F. HOLTON.

THE establishment of a new Prime Meridian affecting all the charts, maps, and gazetteers published, or to be published, in any country, should not be attempted without due consideration. For a long time we have suffered an annoyance, on a most petty scale, from a foolish and futile attempt to reckon American longitude from some unestablished point in the city of Washington, not because it was the site of an Observatory, but merely because it was our National Capital. The abandonment of this attempt now, just when, for the first time, something might be done towards establishing it on a more rational basis, evinces a clear-sightedness as to the real interests and honor of our

nation from which we ought to take courage. For it is seen and acknowledged that the prime meridian is no honor to the place it passes through or from which it takes its name. So, too, it is a badge of honor rather than an honor in itself to the people who established it. If a Prime Meridian be established for convenience, and afterwards the scientific and commercial activity of the people make it a matter of importance to all other nations, it becomes a monument of that activity. But when such a Meridian is established, merely as a national distinction, it becomes a work of vanity, for, however we may value marks of distinction when fairly earned, no one counts it honorable to strive after them.

The Prime Meridian of Greenwich is a badge of honor to us; for we were in our fathers' loins when they established it, and our own commerce and science have aided in giving it its importance. It is ours — as is King Alfred and Shakspeare — and while we hold to it, it is the only candidate for universal adoption. If we adhere to it but one century more, it will remain while the human race endures, the sole prime meridian, a trophy of the superiority of the Anglo-Saxon race.

But it is now proposed to abandon it — to reckon the longitude of Washington as 12° 56′ E. instead of 77° 4′ W., and to keep this assumed longitude, even should we at length discover precisely the width of the Atlantic, and the true longitude from Greenwich. It is further proposed to discover some point exactly 12° 56′ W. of Washington, or as nearly 90° W. from Greenwich as possible, and on that point to establish — not an observatory — but a geodetic signal, which "will be useful for reference in the adjacent country, saving labor and time in fixing longitudes in its vicinity, and whose foundation appears to be peculiarly proper as a national monument."

Would not some elevated point in the everlasting hills of Missouri serve better for a landmark of longitude than any that could possibly be found in the alluvial country south of Wisconsin? And might not the *field* of New Orleans be a better site for a national monument than an arbitrary point in the city?

But we are compelled to admit the necessity of a fixed point from which to number our longitudes, so accurately ascertained among themselves, rather than leave them individually open to a correction of some seconds. There ought to be an American Prime Meridian. Let us inquire, then, where that meridian could best be located. Lieut. Davis's paper shows conclusively that it should not pass through

Washington. It shows also that wherever it must fall, it subjects both sea and land to serious inconveniences. It is even an evil to the continent on which it falls, making some of its longitudes East and others West. Had the French rested content with reckoning Paris as 20° E., or better still, at 30° E., a better prime meridian could not be desired. We may well desire, therefore, to avert from the turbid waters of Barataria Bay the confusion inseparable from a prime meridian. No country, city or people can derive any advantage, honoable or pecuniary, from this unenviable distinction, save only the observatory through which it may pass, and which is thereby relieved from correcting its observations for difference of longitude.

If, therefore, an American prime meridian is to be arbitrarily assumed, let it pass 77° 4′ east of Washington. In this way it will pass through the town of Greenwich, and most probably through the very observatory, though we cannot hope that it will coincide with the axis of the Transit instrument.

To the practical navigator the American meridian will then be regarded as identical with that of Greenwich, and it is only when we speak of American longitudes with the highest degree of accuracy that we consider the two as differing by a small quantity, unknown both in amount and sign. In this way it is to be hoped that every real advantage of a new prime meridian will be secured — every evil incident to it avoided — our scientific community saved from a charge of national vanity — our long-cherished claim to the meridian of Greenwich preserved, and its universality placed beyond all future contingency.

Prof. Holton's paper gave rise to an animated and protracted discussion, in which Prof. PEIRCE, Lieut. DAVIS, and Lieut. MAURY took part in favor of an American meridian, and Mr. G. P. BOND, Prof. LOVERING, and Prof. JOHNSON, took part against it.

Dr. HARE expressed his hope that there might be at some time a universal meridian.

Lieuts. DAVIS, and MAURY explained the views and motives of the Secretary of the Navy, the Hon. W. B. Preston, in inviting the opinions of the mathematicians and astronomers of the country upon this interesting question.

SECTION OF CHEMISTRY, MINERALOGY, AND METEOROLOGY.

J. H. Wurtz, Esq., in the Chair.

Investigation of the Intestinal Fat of a large Sea Turtle. By Dr. Christian Linck, Assistant in the Cambridge Laboratory.

A portion of the subcutaneous tissue of this animal was presented to me by Prof. Agassiz, with the request that I would make an investigation of the nature of its fat. One pound of the substance was repeatedly boiled with water, which yielded from its surface seven and a half ounces of fat, which was purified by fusing and washing it with water several times.

This fat had the consistency of hog's lard, and retained the strong fish-like smell that emanates from all parts of the animal. The color was a light brownish yellow. It easily saponified with a weak ley of caustic soda. This soap solution being supersaturated with sulphuric acid, was transferred into a glass retort, and two thirds of the liquid distilled off. The distilled liquid had the same odor as the distillate obtained from butter under analogous conditions. This liquid mixed with a quantity of baryta water, deposited during evaporation white flakes of the same appearance as the caprylate and capronate of baryta obtained in a similar way from butter. The quantity is apparently so small as not to admit of the isolation of the acid from it in the oily state.

Four ounces of the same fat were saponified, and treated as before; to the distillate a quantity of soda was added, and the whole evaporated to two ounces; this was supersaturated with sulphuric acid and one and a half ounces of liquid distilled over. This gave a decided butyric smell and reddened litmus paper strongly, a reaction destroyed, however, by the addition of a few drops of soda solution, indicating the quantity of volatile acid to be very small. Gottlieb has found precisely the same results in the fat of geese. The fat acids remaining on the surface of the decomposed soap liquor was collected, and treated repeatedly in the same manner as the original fat had been, in order to ascertain whether the formation of the volatile fat acids might not be owing to the reaction of the caustic alkali on one of the main constituents of the fat.

The distillate still possessed the smell of butyric acid, but less and less after each succeeding operation, so that most probably the volatile

acids were formed only during the first saponification, and in the succeeding operations, those parts only which adhered mechanically to the fat after distillation, were given off. The fishy smell of the fat seems to have no connection with the nature of the volatile fat acids, and the substance on which it depends, exists perhaps in no greater quantity than the odoriferous principles of many flowers, which have not yet been isolated.

The fat exposed to the dry distillation until two thirds had gone over, yielded a soft distillate, lighter colored than the original fat, from which emanated the strong and repugnant odor of acrolein, the common product of decomposition of glyceryl, the base which combined with fat acids constitutes animal fats.

This product of distillation was boiled with water for some time, then filtered through a wet filter and set aside. After twelve hours there were deposited crystalline flakes with the appearance of sebacic acid — the principal product of dry distillation of oleic acid.

A quantity of the original fat was saponified with oxide of lead, and the lead plaster digested with ether. Nearly the whole of the plaster proved soluble in this menstruum, showing the presence of a large quantity of oleic acid, the only fat acid whose leadsoap is soluble in ether. The oleic acid was prepared from that soap by decomposition of it with sulphuric acid. It presented the appearance of a brown oil, possessing the physical properties of crude oleic acid. It slightly coagulated when cooled with ice.

It remained now to search for the fixed, solid fat acids. Different methods were pursued, but the following proved the most satisfactory: The mixture of fat acids obtained by decomposition of the original soap, was dissolved in boiling alcohol and set aside for crystallization, the mother-liquor separated by a filter, and the needles which remained purified by very oft repeated re-crystallizations, so that at last their point of fusion did not rise any higher, but remained stationary at 60° C. The product in such state of purity had completely the appearance of hydrated margaric acid, with which it also corresponded in its point of fusion. In this very tedious manner, and with a great loss of substance in the mother-liquors, a quantity of the acid was obtained sufficient for analysis. The soap separated by chloride of sodium, re-dissolved in water, and separated again by sulphate of soda, etc. Part of it was saponified with soda, thus a pure soda soap was obtained, and from it, by double decomposition with

nitrate of silver, an insoluble silver-salt was prepared in the usual manner for preparing these compounds.

Of this silver salt 0.4795 grms. was ignited, until all organic matter was destroyed, 0.1360 grms. = 28.36 per cent. of pure metallic silver, remained behind.

Varrentrapp obtained from pure margarate of silver, 28.20 per cent. of silver, which agrees very well with the result which I have obtained.

By a combustion with chromate of lead, 0.3241 grms. substance yielded: Carbonic acid = 0.8955 grms., and water 0.361 grms., which makes the composition of the substance: Carbon 75.35 per cent.; hydrogen 12.37 per cent.; oxygen 12.28 per cent.

Varrentrapp's and Sacc's analyses vary between 75.30 and 75.85 per cent. carbon, and 12.22 and 12.69 per cent. hydrogen. These analyses prove that the solid acid in the fat operated upon, is identical with margaric acid. The fat could not contain any stearic acid, because if so, this would have accompanied the margaric acid throughout the process of its preparation, and would have raised its point of fusion above 60° C.

The soap which the original fat forms with soda, has a yellow tinge, which can be removed mostly when the soap is separated by salt out of a strongly alkaline solution. The alkaline mother-liquors are then of a deep brown color, and on super-saturation with an acid, deposit a small quantity of a brown sticky precipitate, which is soluble in alkali or in alcohol, and seems to consist of fat surcharged with coloring matter.

As the result of this investigation, it appears that the fat under examination consists of margarate and oleate of glycirin and a small quantity of the volatile acids of butter; it contains no stearic acid and shows great analogy with the fat of geese.

On an Instrument for Determining the Variation and Duration of Winds. By Prof. James H. Coffin.

The Report of the Regents of the University of the State of New York, for the year 1839, contains a description of an instrument that I formerly used to measure the duration of winds from the several points of compass. A vane was attached to the top of a perpendicular shaft, and at the bottom of the shaft a funnel-shaped tube was fastened pointing obliquely downward, so that as the shaft revolved by the action of the wind on the vane, the smaller orifice of the tube would describe a horizontal circle. A small stream of sand, gauged

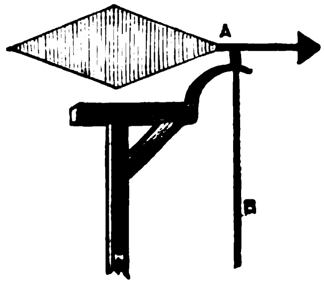
in the same manner as in an hour glass, was constantly running into the tube at the upper orifice, and thence descending to the lower orifice, was distributed into different boxes, (thirty-two in number,) according to the direction of the wind. The quantity of sand collected in each box, showed precisely the length of time that the wind had blown from the corresponding point of compass.

The foregoing instrument, which I used about a year and a half, answered a very good purpose, but there were two defects in it. 1st. While it recorded the *duration* of the several winds with great minuteness, it failed to inform me *at what hour and minute they occurred,* a point of considerable importance in connection with the study of storms, and several other meteorological phenomena. 2d. It required attention twice a day to replace the sand.

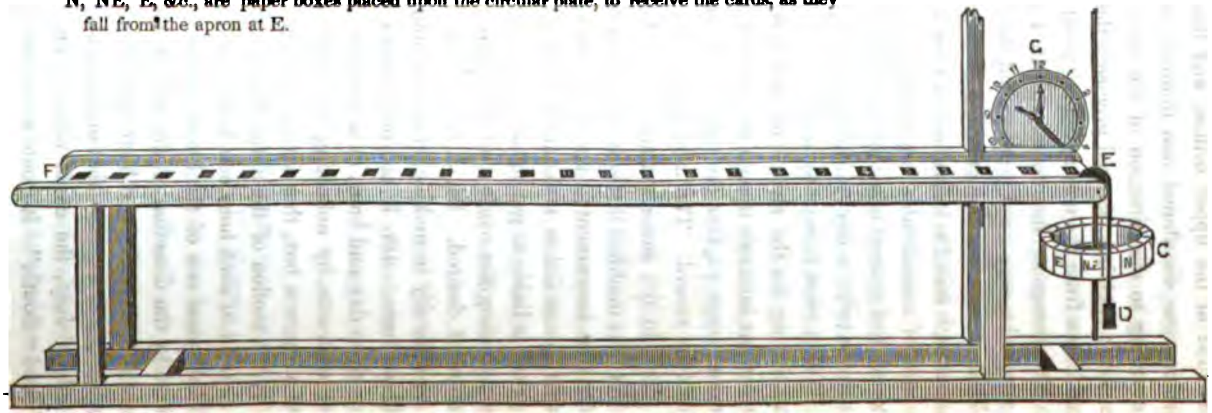
With a view to remedy these defects I modified the instrument, by substituting for the stream of sand a row of minute cards, arranged at regular intervals upon a movable band or apron, each card having printed upon its face the number of the day and hour upon which it made the record. The motion of the band or apron was regulated by a clock, in the same manner as in the animometer of Osler.

As thus modified it seems to me to possess the following advantages over that instrument: 1st. It operates with more *certainty*, and as *cards* cannot fail to make their records at the proper time, whereas a *pencil* is liable to get out of order. 2nd. Its records are more *definite*, dividing the winds into thirty-two distinct classes, or a greater number if desired. 3d. It *requires attention less frequently*. There is no difficulty in making it keep an hourly record for months together without care. 4th. It not only shows the direction of the wind on any given day and hour, as is done by Osler's, but it also *prepares its own abstracts*, by collecting the records for each point of compass into a separate box, thus saving much labor in reducing them.

By the motion of the apron the cards are carried forward, so that at the end of each hour one arrives at the roller E, (see Fig.) and falls off into some one of the boxes below, the particular box being determined by the direction of the wind at the time. As each card shows upon its face the day and hour upon which it fell, the record is complete. For example, suppose that on examining the boxes at the end of the month of July, the cards which I enclose were found in the box labelled "South." It would show that there was a south wind on the 3d day, 18th and 20th hours; on the 18th day, 12th hour; on the 22d day, 24th hour, &c.



A, is the Vane.
B, is the Perpendicular Shaft.
C, is a Horizontal Circular Plate of light material attached to the shaft.
E and F, two Rollers communicating motion to the Apron E F from left to right.
1, 2, 3, &c., are minute Cards, placed upon the Apron.
G, is a Clock that regulates the motion of the Roller E, and consequently that of the apron and cards.
D, is a small weight to relieve the Clock.
N, NE, E, &c., are paper boxes placed upon the circular plate, to receive the cards, as they fall from the apron at E.



In an instrument of this kind that I set up at an observatory on the summit of Saddle Mountain, near Williams College, and which was designed to run four months without care, during the colder part of the year, when the mountain is inaccessible, the apron was necessarily too long to pass around rollers in the manner represented in the figure, and it was therefore wound upon drums. But when the records can be transcribed as often as once a week, the form given in the figure is preferable.

I may remark farther, that the *force* of the wind may be registered in the same way, by means of an additional row of cards placed upon the apron.

Seventh Day, August 21, 1849.

SECTION OF NATURAL HISTORY, ZOÖLOGY, &c.

ON THE STRUCTURE AND HOMOLOGIES OF RADIATED ANIMALS, WITH REFERENCE TO THE SYSTEMATIC POSITION OF THE HYDROID POLYPI. BY PROF. LOUIS AGASSIZ.

THE importance of the study of homologies has been fully exemplified by the results obtained from a thorough comparison of the different classes of vertebrated animals. A deep insight into their structure has been gained since we knew that their different parts, as fins, wings, and legs, are only modifications of the same organs, undergoing different developments in the different classes. The homologies of the skeleton have been particularly investigated, and those familiar with these researches know that they have led to a thorough understanding of the typical peculiarities of this system of hard parts.

Less extensive, though not less valuable investigations have been made upon the muscular as well as upon the nervous system, with equally valuable results.

Besides the more accurate knowledge of organs thus obtained, another important conclusion has been derived from these studies. It has been shown that the functions of certain parts cannot altogether be relied upon in order to ascertain their true nature. For instance, it was a mistake to identify the lungs and gills among *Vertebrata*, and to compare the aerial respiratory organs of mammalia, birds, and reptiles, with the gills of fishes, since there are reptiles in

which we find both kinds of apparatus, existing simultaneously throughout life, and performing at the same time the same functions. This shows that in the investigation of the structure of animals, we should distinguish between homologies and analogies, just as well as we distinguish between affinities and analogies, where we investigate the natural relations between animals.

The whales have no affinity with fishes beyond the general one which exists between all *Vertebrata*, but they are analogous to the fishes in their general form, and by the fact that they inhabit the same element.

The bats are analogous to birds in being provided with wings to fly through the air, but they do not belong to the same class; their nearest relation is with the mammalia.

In the same manner should we distinguish between the different kinds of apparatus which perform various functions, as I have already pointed out, in mentioning the gills and lungs. But at the same time, it is important to ascertain the structural relation, or the homology which exists between kinds of apparatus performing different functions. As for instance, the legs of quadrupeds, the pectoral and ventral fins of fishes, and the wings and legs of birds, which are truly homologous, while their lungs and gills are only analogous to each other.

This first general result will presently lead to another very important investigation. It is a matter of great interest to naturalists, to ascertain how far the parts which perform similar functions in the different great groups of the animal kingdom, are homologous to each other, and how far they are simply analogous. Is it true, for instance, that the legs of insects, crabs, lobsters, and worms, correspond truly to the parts which we designate by the same names among vertebrates? Are the wings of insects identical in structure with the wings of birds? Or, are these parts only analogous? Can the respiratory apparatus in *Articulata*, their gills and tracheæ, their various air sacks, be compared to the gills and lungs of vertebrates? Is the heart of *Crustacea* the dorsal vessel of insects? Are the vesicular sacks of worms really hearts in the same sense as we distinguish a heart in vertebrates?

Again, are the gills and lungs of *Mollusca*, their various apparatus of locomotion, identical with the similar parts in *Vertebrata* and *Articulata*? And can we trace correctly such a comparison among *Radiata*?

This is the question which I propose to answer in this investigation

upon the Homologies of Radiated Animals. Before proceeding to the illustration of their structure in the details which I hope will show that such an homology as our present nomenclature in Zoölogy would lead to infer, cannot at all be sustained upon any philosophical principles; let me first show that even the general form of the animals belonging to the different great types of the animal kingdom does not agree, and that it is neither correct nor advantageous to the progress of science to use similar expressions to designate their various parts as has been usual among naturalists, ever since comparative anatomy aimed at illustrating the peculiar structure in all the classes of the animal kingdom. Radiata, are animals whose prominent character rests in the peculiar outline of their body, as well as the peculiar relative position of their parts. Whatever may be the modifications which they present in different classes and families, they can all be reduced to a typical form, best expressed by a star, or a circle, with an internal radiated arrangement of the parts. And all animals, whose structure cannot be reduced to such a diagram, must be removed from this great group.

That intestinal worms do not truly belong to the type of *Radiata*, as Cuvier admitted, has been satisfactorily shown, by the investigations of all those naturalists, who have studied the subject more recently. That *Infusoria* again, do not constitute a natural group, is best exemplified by a comparison of the different families which have been combined under that name.

The *Rotifera*, for instance, can plainly be shown to belong to the class of *Crustacea*, as the closest homology can be traced between them and the *Malacostraca*. I have recently shown that a great many of the *Polygastrica* are simply larval forms of different worms, especially those of the type of *Paramœcea;* whilst others, such as the *Vorticellæ*, truly belong to the *Bryozoa*, and are therefore true *Mollusca*, and a great many others, especially the *Polygastrica anentera*, are truly vegetables.

But even after removing these classes from among *Radiata*, we find several groups left in the class of *Polypi*, which are neither true *Polypi* nor really *Radiata*. Such are, for instance, *Foraminifera*, which I have recently ascertained to be the strongest resemblance to the embryonic forms of *Gasteropoda*, and to be therefore a low form of gasteropodian mollusks, whilst the *Bryozoa* constitute a natural group, closely allied to the tunicated mollusks, and therefore belong to the class of *Acephala*.

Again, the so-called *Hydroid Polypi*, do not in my opinion, belong to the class of the *Polypi*, but should constitute a special family among *Medusæ*, to which they bear the same relations as the *Crinoides* with their stems, bear to the free-moving starfishes. But in order to show that these views are strictly correct, it is necessary for me to illustrate first the homologies of *Radiata* in general, as well as the organic characters of their different classes. While it will be obvious that the presence or absence of a stem is no more a peculiar character of *Polypi*, than of any other classes, as we have free *Polypi* among the *Actiniæ* and fixed *Echinoderms* among the *Crinoids*, and there is no reason why there should not be also fixed *Medusæ*, provided their true structure agrees more closely with that of the true free *Medusæ*, than with that of *Polypi*.

The relation of *Hydroid Polypi* to *Medusæ* has already been alluded to by me in my lectures on Comparative Embryology, but I hope here to show more fully that this view is sustained by the closest investigation of their special homologies.

Echinoderms, *Medusæ*, and *Polypi*, restricted as above mentioned, constitute, in my opinion, the only true representatives of the type of *Radiata*, and form, in this limited connection, a most natural group in the animal kingdom, which agree entirely in their radiated structure, and differ only in the manner in which the structure is developed in the different classes.

In *Polypi*, the body constitutes a simple sack with internal radiated partitions and a central aperture at one extremity surrounded by fringes and communicating with an inner sack which itself empties into the main cavity of the body.

In *Medusæ*, the main cavity of the body is not sub-divided, but limited by a large development of the substance of thin wall, and from this central cavity arises a system of tubes which divide in a radiated arrangement into that substance. The mass itself projects more or less in the form of an umbrella or of lobes with various fringes around their margin; whilst the mouth projects from the centre surrounded by various loose appendages, or in the form of a proboscis arising from a more intimate connection of the same parts which in others constitute the appendages of the mouth.

Compared with the *Polypi*, therefore, the *Medusæ* present this peculiar difference, that their mouth is prominent, more or less tubular, and provided with a peculiar fringed apparatus, whilst in *Polypi* it is in one level with the walls of the body, and simply folded at its open-

ing. So that, if any comparison can be instituted between them, we shall only trace an homology between those folds and the appendages of the mouth in *Medusæ*, and not between those and the tentacles of *Polypi*. On the contrary, the tentacles of the *Polypi* are homologous to the fringes, or various appendages which hang from the margin of the disk of true *Medusæ*. On the other hand, in *Polypi* the internal cavity is divided into two distinct spaces, one circumscribed with peculiar walls, and forming a kind of stomach, and the other below formed simply by the external walls of the animal itself, from which arise partitions which sub-divide partly this lower cavity into open cells, communicating above with the cavity of the tentacles, which correspond in number to the number of the partitions.

In the *Medusæ*, on the contrary, the main mass of the body is solid, nd traversed by a regular system of radiating tubes, communicating at the periphery with a circular tube extending all round the margin of their disk. And the cavity of the stomach communicates with this system of tubes by a few openings, and not by a single large central one as in *Polypi*. Notwithstanding the fundamental difference which this arrangement introduces between these two classes, it cannot be overlooked that the system of tubes of *Medusæ* is homologous to the partitions of *Polypi*, but constitute a lower condition of a similar apparatus, as in *Medusæ* the tubes are regularly ramified, and again combined by a circular canal, whilst in *Polypi* all the partitions communicate freely with each other in a central general cavity, and open freely into the tentacles, and even through the tentacles with the surrounding medium.

Moreover the sexual apparatus of *Polypi* consists in bunches of eggs, or in spermatic chords attached below the stomach to the free margins at the partitions in the main cavity of the body, whilst in *Medusæ*, especially in those which resemble nearest the *Hydroid Polypi*, the sexual apparatus forms bunches of eggs or of spermaria between the outer circle of tentacles and the fringes surrounding the mouth,— sometimes more closely connected with the disk itself,—sometimes adhering with the outer surface of the proboscis. And now in *Hydroïd Polypi*, especially in *Tubulariæ*, the bunches of eggs hang from the base of the external surface of the proboscis within the circle of tentacles which surround the main mass of the body; an homology which establishes the closest relation between true *Medusæ* and *Hydroid Polypi*. Indeed, it can now be said that the so-called *Hydroid Polypi* are true *Medusæ*, resting upon strings, and not

merely polyp-like animals producing alternate generations of various free *Medusæ*.

The intimate connection which exists between *Hydroid Polypi* and *Medusæ*, which has been so beautifully established by the investigations of Sars, Steenstrup, Liebold, Loren, Du Jardin, Sir John Dalyell, and others, might have been ascertained long ago, merely upon homological evidence, if the fact of their resting upon a stem had not constantly misled observers to consider them as *Polypi*.

The conclusion of these remarks allows me therefore to state, most decidedly, that *Hydroid Polypi* are true *Medusæ*, resting upon stems. The homologies thus traced have led to some other discoveries of minor importance.

Thus, the position of the eye specks around the margin of true *Medusæ* between their tentacles, pointed to the place where similar organs should be searched for among other *Radiata*. They were accordingly discovered between the tentacles of *Lucernaria*, in a position identical with that which they maintain in Aurelia. This fact will show once more how many material additions to our knowledge of the structure in the details of animals, may be obtained by tracing their more general relations. It is somewhat more difficult to trace the homologies between *Echinoderms*, and *Medusæ* and *Polypi*, as the first of these classes is so much more complicated in its structure than the two others. However if we consider only the general relations of their organs, we shall experience no difficulty in comparing them.

In the first place, the external wall of the body in *Echinoderms* assumes a highly complicated structure, by the peculiar development of isolated calcareous plates, arranged in a most admirable symmetrical order, and affording by their combinations a solid support, and points of attachment for most of their organs.

But however complicated these walls may be, and I need only refer to the works on Comparative Anatomy for the details, it is obvious that these walls which enclose the general cavity of the body correspond to the gelatinous mass of *Medusæ* and to the membranous walls of *Polypi* with their solid parts. The mouth, which is soft in the other classes, is here frequently surrounded by solid parts and even by a complicated system of jaws and teeth, surrounded by simple fleshy lip.

The isolated fringes on the margin of *Polypi* and *Medusæ*, are transformed here into complicated gills, protruded between the solid covering, and the membranous covering of the mouth; and these gills are truly homologous to the tentacles of *Polypi* and *Medusæ*;

we need only remember the fringed tentacles of some of the *Actiniæ* to be satisfied of their identity. The inner gills with their ambulacral pedicelli are foreshadowed in *Polypi* in the minute vertical rows of pores, which are observed on the outer surface of the *Actiniæ*, between the partitions. Similar openings reappear, nevertheless, in *Echinoderms*, in the form of a peculiar system of waterpores.* The greatest progress in the complication of structure in this class, however, is shown in the development of the digestive system and its appendages, which constitutes here a regular alimentary canal, with various divisions generally accompanied with a complicated system of bloodvessels. The sexual organs also grow more independent, constituting a distinct system with special openings, and indeed the prominent structure of *Echinoderms* may be said to consist in the localization of the different apparatus upon some few of their rays, whilst there are as many radiating structural appendages as there are rays in the lower types.

In *Polypi* the number of tentacles is sometimes very great. In *Echinoderms* there are only five bunches of gill-like fringes. The ovaries are nearly as numerous in *Polypi* as the inner partitions; in *Echinoderms* there are only five, sometimes only four, and even only two ovaries. In *Polypi* the cavity in which the nutritive fluid moves is sub-divided by partitions into many open cells. In *Echinoderms* the homologous system constitutes a cylindrical simple tube, or sack, which branches only in the lower forms of that class, as in the starfishes, where numerous liver-like appendages are also attached.

The circulating system in *Polypi* is directly connected with the alimentary sack, whilst in *Medusæ* it is more closed in itself, especially in the *Ctenophora*, and in *Echinoderms* the distinction is carried out still further.

The nervous system where it is well developed in these animals, forms a ring around the entrance of the alimentary canal with radiating threads to the different rays terminating at the eye specks.

Among *Medusæ*, the nervous ring presents a similar arrangement, though this system is less distinct in its substance, a circumstance which leads me to suppose that our efforts to discover a peculiar nervous system in *Polypi* may be altogether vain, as it is possible that their sensitive faculties are concentrated in these animals in a particular set of cells without peculiar distinct connection. From the compari-

* See my paper in Comptes-rendus, 1847.

son traced above, it must be plain that the three classes of *Radiata*, restricted as they are here, constitute a natural, well defined group in the animal kingdom, and that the separation of the *Echinoderms*, as one great type of the animal kingdom, equivalent to that of *Mollusca* or *Vertebrata*, as Dr. R. Leuckart * would have them, rests upon an exaggerated estimation of their peculiarities. There is, however, one more general question to examine respecting the relations of this great type to the other main groups of the animal kingdom, namely, the *Mollusca*, the *Articulata*, and the *Vertebrata*. Can the walls of *Echinoderms*, *Medusæ*, and *Polypi*, as they surround the body in their various complications be compared with the walls forming the main cavity of the body in *Mollusca*, *Articulata*, and *Vertebrata*? Certainly not beyond the general fact, that they arise in a similar manner from the substance of the egg, but in their special adaptation there is a complete difference.

In the *Radiata* we have no right and left, no above and below, no anterior and posterior region. The body is a mere sack, either with membranous, or gelatinous, or diversified walls ; it is a mere sack or sphere. Their solid parts where they exist, agree, neither with the shells of *Mollusca*, nor with the solid joints of *Articulata*, nor with the skeleton of *Vertebrata*.

Again, their alimentary apparatus even where it forms a tube, has neither the same relation to the respiratory apparatus, or the system through which the fluids are circulated ; nor does it stand in the body in the same general connection with its main cavity. The fringes around the mouth, or around the disk, as well as the so-called gills of *Echinoderms* are not gills with the same connection with the circulatory system as in other animals. They are either directly connected with the main cavity of the body, or their peculiar openings with the surrounding media. The same may be said of the nervous system, of the sexual apparatus, &c., &c., so that the designation of these systems by the same means as we employ for other classes, should be only used with the distinct understanding, that they relate to analogous systems, but that no special homologies can be had between them.

* Morphologie der Wirbelloseen Thiere, 1848.

On some Plants of the Order Compositæ from the Sandwich Islands. By Prof. Gray.

Prof. Gray made a brief communication on some characteristic plants, of the order *Compositæ* in the Sandwich Islands, namely, on *Argyroxiphium Sandwicense*, *DC.*, of the island of Hawaii, and *A. macrocephalum*, a new species, gathered by the naturalists of the Exploring Expedition under Capt. Wilkes, and which represents this remarkable genus on the island of Maui. He referred this characteristic genus to the division *Madieæ*, which is so largely represented in California, and is entirely restricted to the Pacific border of the American continent, reaching from Oregon to Chili. He remarked that the new species differed from the original *A. Sandwicense*, just as the California *Lasthenia glabrata* does from *L. Californica*, as *Burrielia chrysostoma*, *Ptilomeris calva*, and several other California Compositæ differ from the type of their genera, namely, in the suppression of their pappus. *A. macrocephalum*, besides its larger capitula and narrower leaves, bears achenia which are entirely destitute of pappus, although in every other respect it is a strict congener of the original species. Thus far, these plants illustrate the rule, noticed by Dr. J. D. Hooker, in respect to the Galapagos Islands, that when a genus of two or more species is peculiar to a group of islands, the different islands have each their peculiar species.

In farther illustration of this law, Dr. Gray exhibited specimens of a third, nearly allied plant of the order, from the Island of Kauai, where it manifestly represents the genus *Argyroxiphium*, but constitutes a very distinct genus in character, allied as much to *Lasthenia*, as to the former genus. After noting the characters of this genus, Prof. G. remarked, that, as it was one of the most striking and interesting new plants collected during the cruise of that successful expedition, he proposed to characterize it, in the portion of the botany of the voyage he had now in preparation, under the name of *Wilkesia*; thus dedicating it to the enterprising commander of the expedition. In order to indicate its affinity with *Agyroxiphium*, he proposed to derive the specific name from the nakedness of the sword-shaped leaves, which present only slight indications of the silky covering, and thus to call it *Wilkesia gymnoxiphium*.

Prof. Agassiz said Prof. Gray had alluded to two general points of interest. It was a remarkable point that, at some times, some particular branches were in advance of others. Structural differen-

ces were often found to mark geographical divisions, both in animals and plants. But there was a point in which the zoölogical researches are behind those of botany; and that was, in respect to particular species being confined to specific geographical localities. He had no doubt that this was as true with reference to animals as to plants.

On the Embryology of Planariæ. By Ch. Girard.

The published Faunas of this country do not yet make any mention of the group of *Planaria*, although Prof. Haldeman has described the first species nine years ago.

These animals inhabit salt, brackish, and fresh water, each of these localities having peculiar forms of that family and characteristic species.

Being at this moment engaged in elaborating a monograph of the North American Planariæ, which I propose to publish shortly, I shall confine myself to-day to an exposition of the general results of embryological observations made upon one of the species.

It belongs to the genus *Planocera* of De Blainville, and I shall designate the species under the name of *Pl. elliptica*, on account of its elliptical form.

As early as the month of December, I observed in the interior of the body within the gastro-vascular system, little semi-transparent spheres distributed into all parts, from one extremity of the body to the other; these are the eggs in process of formation. The germinative vesicle is very distinct, as well as the germinative spot; the first is proportionally large, the second very small. The substance of the vitellus, although there is but a small quantity present, is nevertheless distributed throughout the whole interior of the sphere circumscribed by the vitelline membrane.

This vitellus is homogeneous in appearance, and is composed of little cells, which are scarcely to be distinguished from those of the tissues of the body, except that they appear smaller, showing thus an analogy alluded to by Prof. Agassiz in his Lectures on Comparative Embryology. (p. 81, &c.)

The deposition of the eggs takes place during the months of May and June. Sometime before this period, the egg has become opaque by the augmentation of the vitelline substance, which increases in proportion as the egg grows larger, and completely conceals the ger-

minative vesicle, which shows itself no more except upon compression. The germinative spot has already disappeared.

When the animal deposits its eggs, it fixes them upon smooth surfaces by means of a thin layer of a sort of mucosity which hardens upon contact with the water. They are disposed side by side in bands of a quarter of an inch in breadth, the length of which varies according to the quantity of eggs deposited at one time. The same individual deposits eggs many times during the same season, at short intervals, and constructs each time one or many bands.

When the egg is laid, it has an external envelop, the chorion, between which and the vitellus, a small space is left. The vitellus itself is completely opaque; very rarely, it is true, there is still to be seen upon the surface a transparent space; this is the germinative vesicle, which is at the point of disappearing also, as the germinative dot has already done. At this period, the vitellus is composed of fine cells, each little cell containing a nucleus of variable size.

About twelve hours after the deposition, the egg enters into a new phase, that of division, which lasts twenty-four hours. The phenomenon takes place in the following manner: — The vitelline sphere elongates into an oblong form, a concentration appears at the centre, at first very slight; afterwards becoming more and more pronounced until the separation into two parts is complete. The primitive sphere divides thus into two smaller spheres of equal size, in the centre of which is seen a transparent space which has no connection with the germinative vesicle, as Prof. Agassiz showed for the first time last year, in the nemertian worms, and as we have also frequently verified since.

Soon after, two other spheres appear opposite each other on the sides of and between the two primary anterior ones. At first very small, they increase rapidly until the whole is divided into four equal parts, four spheres independent of each other, but closely grouped together, and having each a transparent space. I have said that those spheres were independent of each other; I could convince myself of it directly, by isolating them under compression without breaking them. They can be isolated when they are very numerous as well as when there are but two. This fact leaves no doubt that a membrane surrounds each sphere of division. At this period the vitellus is heterogeneous, consisting of large and small cells, containing one or several nuclei; the latter of different sizes, sometimes opaque, sometimes with a transparent centre.

After these four spheres, four new ones are formed alternating with the preceding; next, eight others, which alternate also, and so on. This law of division by doubling the number preceding, is well known to embryologists. The respective arrangement of the spheres of division is subject to another law, namely, that one which causes them to alternate. This law was discovered by Prof. Agassiz, upon these same eggs.

Beyond sixteen divisions this regularity is less apparent, and at thirty-two and sixty-four it can no longer be traced; finally, the mulberry-shaped body appears, which is the last stage of division, and which, after six hours, brings us back to the primitive sphere, with a cellular structure very similar to what it was before the division.

One fact clearly appears from these observations, namely, that during each of the changes which the egg undergoes, the vitelline substance is under the control of a very subtle elaboration which transforms its mass, kneads it and re-kneads it, so to speak, again and again at each entry into a new phase. The embryonic substance multiplies itself by the most simple and unexpected process, which at once shows the fallacy of the cellular theory, as it has been admitted up to the present time by embryologists. It consists of *the growth of the nuclei.*

Prof. Agassiz has himself illustrated in one of the past meetings the new theory with which he proposes to replace the old one. I say old, and yet it has only existed for about ten years. But such has been the rapidity of the progress in these investigations that it has lived long in a short time. The one which is called to succeed it rests upon such numerous facts and observations repeated so many times, (for all our microscopial studies conform to it,) that I have no doubt it will remain unshaken by subsequent investigations.

The division once completed, the eggs remain in a state of repose for four or five days, during which, their mass becomes more transparent and their centre hollow. At the end of this time the germ begins to move.

This movement is not uniform with all; in some it is very rapid, nearly forty turns a minute. With others more moderate, fourteen turns only a minute; with others, again, still slower, and sometimes it was scarcely sensible. Those which moved slowly one moment, would the next instant move with great rapidity, and, *vice versa*, those which moved rapidly could decrease their motion; in a word, the same germ is subject successively to different degrees of velocity.

The movement does not take place in a uniform direction. Some turn from right to left, others from left to right in a horizontal plane; others, again, turn in a vertical plane, sometimes uniformly, at other times with a bounding movement.

Both of these movements will change in direction, that is to say, if the germ turned from left to right it will change and turn from right to left in the same plane.*

Great vitelline cells, independent from the principal mass, — cells which Prof. Nordmann has described in the Phlebenteric Molluscs as parasitic animals, under the generic name of *Cosmella*, — circulate freely in the space between the vitellus and the outer membrane of the egg. When the mass moved rapidly, these isolated cells were attracted and carried irresistibly away in the current, like satellites around a large and powerful planet.

It was an attractive sight, which we could not be tired in observing — such activity in a spherical mass of cells — a globe of matter under the impulse of life, which was to govern it, and in some sort imprint upon it the forms which an overruling Will had marked out beforehand, and from which matter does not deviate.

This spherical mass of cells is already the embryo. Twelve hours after, the centre of this embryo, which is always circular in form, is composed of large transparent cells, the contents of which cells are nebulous, and around them are found the ordinary vitelline cells. At this time, the exterior surface is seen to be covered with vibratory cilia. I have observed no trace of these organs, at the moment when the embryo begins to move; and they do not become apparent until the day following. After the lapse of another twelve hours, the embryonic sphere loses its primitive form, in consequence of the flattening of one of its poles; and from this surface rise up several tubercles, between which a groove is formed, and the sphere opens.

From this moment the embryos are symmetrical, which can be directly seen by the appearance of the eyes. Being still in the egg, the movements are confined within the circumference of the external envelope, where they seem to be uneasy unless they are moving in a shaking and irregular manner.

* The expressions *vertical plane*, and *horizontal plane*, have reference only to the observer, as the sphere can displace itself from one plane, and assume the other. The irregular movement of some of the germs, which appear at times to oscillate, and move in a spiral direction, must no doubt be attributed to the passage of the egg from one of these planes to the other.

At last, they break the envelope of the egg, and move freely about, with various movements, and very different forms. They move forwards, but still turning around their axis, and at the same time rolling or oscillating. Their form is sometimes regular and symmetrical, sometimes unsymmetrical, polymorphous, or deformed; they are seen to change it, as well as their direction. The embryos are endowed with a plastic elasticity; they pass from one form to another, assuming them successively without arresting their movement, although each form causes a modification in the mode of progression.

At this stage of development, larvæ of *Planaria* have been described as *Infusoria*, like many other germs. The so-called, *Kolpoda cucullus*, is one of the embryonic stages of a species of fresh water *Planaria*, as Prof. Agassiz has already shown.

These different forms, subjected to such various movements through which the young *Planariæ* passes, become, at the end of eight or ten days, a kind of chrysalis, completely immovable, having a cylindrical form, slightly curved, in which three regions can be distinguished — the anterior and posterior region, both of which are opaque, and the central, which remains transparent.

So few now remained of the larvæ which I had raised, that I was unable to continue my investigations. At first, I thought that this state of chrysalis was an abnormal one, owing to their captivity; but Prof. Agassiz and myself have been fortunate enough to observe them in the same state in Massachusetts Bay, opposite Cape Cod, taken directly from the bottom, in deep water, by means of the dredge.

There cannot be a doubt, therefore, that the young *Planariæ*, after leaving their Infusorial condition, pass a part of their life in the form of chrysalis. How long they remain so, I am ignorant. It remains, therefore, to be investigated, whether the *Planariæ* are born directly from this chrysalis, or whether there are still other intermediate phases to pass through before the perfect animal is produced.

On the Urodelian Batrachians. By Prof. Baird.

A verbal abstract only of this paper was made by the Professor.

Observations on Leuciscus pulchellus, Lt. By W. O. Ayres.

Few attempts have been made, in the accounts of fishes which have been published in this country, to describe the changes which

they undergo at different ages, or at different seasons of the year, or to illustrate their habits. Each description has generally consisted of a statement of the dimensions and external appearance of the fish, and from such imperfect notices numerous errors have arisen. The object aimed at in this paper is, to bring forward what we have learned as to the history and habits of this beautiful species; to give descriptions in detail of its external characters at all stages of its growth, accompanied with a full anatomical account; and to compare it with those species to which it is most nearly allied.

The geographical range of the *Pulchellus* does not appear to be extensive. It is known to exist throughout New England, and is included by Dr. De Kay, among the fishes of New York; further than that, we have not traced it. It has not been seen by Dr. Kirtland in Ohio, by Prof. Baird in Pennsylvania, or by Dr. Holbrook in South Carolina.

It is exclusively a fish of the running water. Wherever a stream which they inhabit is dammed, none but very small dace are found in the still water of the pond, and even they do not seem to thrive. And in the small lakes, which are so numerous, this species is not a native. But though the rivers seem thus their appropriate place, we seldom find them in brooks; they are there very generally replaced by two smaller species, *Cornutus* and *Atronasus*.

The spawning of the *Pulchellus* takes place principally during the months of April, May, and June. At this season, the larger fish ascend the tributaries of the main rivers in much greater numbers than at other times. The young appear to grow with different degrees of rapidity. At the opening of the spring, we find the fish of the last year, in some instances, four inches long, while many may be seen not more than two inches in length, though this is undoubtedly owing in part to the different periods of spawning in the parent fish. Beyond this age, their growth varies much. My observations lead me to conclude that in favorable situations, their weight at the age of three years, is between a pound and a pound and a half.

In size, this species is certainly in advance of any other *Leuciscus* which has been described as inhabiting this country, excepting the *cataractus*. Specimens have been taken at Hartford, Conn., which weighed three pounds. The *cataractus*, according to Prof. Baird, attains a length of two feet, and a weight of four pounds.

The food of the *Pulchellus* is much varied. They take most commonly insects, shell-fish, young fish, and earth worms; I have

found in their stomachs fragments of leaves and locks of wool, and in confinement they feed freely on moistened Indian meal.

This species is liable, during the warmer months of the year, to the growth of a species of *Conferva*, on its scales; this has never, I believe, been mentioned in connection with the fishes of this country, though it is not uncommon on several species — it has been noticed on many of the fluviatile fishes of Europe. It shows itself in the form of grayish patches, resembling tufts of moss.

A singular fact in relation to the *Pulchellus*, is the effect produced on it by the seeds of *Menispermum coculus*, known as *Coculus indicus* or *cockle*. This, when eaten by the fishes, causes them to rise to the surface intoxicated, in which condition they are easily caught as bait for other species. The effect is accomplished, during summer, in fifteen or twenty minutes. As the weather grows colder, the same result requires three or four hours, owing doubtless, to the diminished rapidity of the circulation. It is most remarkable that the poison of the cockle seems to be entirely inert, when administered to any fishes except the *Cyprinidæ*.

The descriptions of both external and anatomical characters, which form the greater portion of the original paper, of course cannot be given in this abstract. One or two points, however, may be deemed worthy of notice.

The *Pulchellus* passes through a regular series of changes in color. The fish of two inches has the back greenish olive, with the edges of the scales darker, these darker edges forming decussating lines, which on the back are very distinct, but on the sides become less apparent, and beneath the lateral line entirely disappear. A broad black band runs the whole length of the side, and above this, a narrow golden line. All beneath is silvery.

At the length of six inches, the green of the back has become less pure, being mixed with brown; the decussating lines extend further downward, the black band has disappeared, and the sides are entirely silvery. At this age, it is the *Argenteus* of Storer.

From this size, it begins to assume the colors of the adult fish, and the change is sometimes complete at the length of ten inches. It is then dark greenish olive on the back, with the edges of the scales almost black. Broad black, decussating lines extend far down the side; these lines are formed by a black deposit upon the fold of skin which encloses the base of each scale. The sides are silvery, strongly tinged with yellowish flesh color. At all ages, the black

band which is characteristic of the young, may be indistinctly seen while the fish is living.

One change still remains, more striking, perhaps, than the others; it is that of the male, at the spawning season. The back is of a dark blackish brown, with the sides strong reddish flesh color. The abdomen is of a pale flesh color, but appears very dark from being covered with minute black dots; the pre-operculum is dark purplish red. At this season, also, the head is furnished with a great number of hard, horny tubercles, firmly fixed to the skin.

It may here be remarked, that the black longitudinal stripe on the young, and the tubercles on the head of the male, are found in many species of *Leuciscus*, and other allied genera.

A single point in anatomy will be mentioned. In relation with the inferior pharyngeals, we find two minute bones, very simple in their structure, but quite remarkable in the service which they perform. They are suspended vertically in front of the pharyngeals, below the branchial arches, being articulated by their superior extremity upon the second interbranchial bone, one on each side. These little bones are slightly curved inward, and are very slender; in a fish of eleven inches, they are about a sixth of an inch in length, and not thicker than a fine sewing needle. The inferior extremity is free, but is provided with a slender tendinous slip which passes upward and forward, and is inserted near the anterior extremity of the second interbranchial; by means of these tendons, which act as braces, these bones are enabled to become places for attachment of a pair of muscles, whose operation is to separate the inferior pharyngeals. These little bones are found, apparently, in the whole genus *Leuciscus*, perhaps in the whole of *Cyprinus*, but not in *Catastomus*, though the arrangement of the pharyngeal apparatus in that genus is very similar to that of the *Leucisci*.

The species of our own waters, with which the *Pulchellus* may be compared at its different stages of growth, are *Catastomus tuberculatus*, *L. atronasus*, *L. americanus*, *L. cornutus*, *L. pulchelloides*, and *L. cataractus*. Prof. Baird informs me, that he has other closely allied species, but they are, as yet, unpublished. Of European species, the one which appears nearest to *Pulchellus*, is *L. vulgaris*. Others are *L. dobula*, *L. lancastriensis*, and *L. albus*.

Prof. Baird, in reply to some remarks by Mr. Ayres, on this fish, claimed for Pennsylvania the existence of a larger allied species,

(*Chilonemus cataractus*, *Baird.*) He had seen individuals from twenty to twenty-four inches in length, and weighing as much as four pounds.

Dr. JACKSON presented the following letter:

Milwaukee, Wis, *August 7th*, 1849.

DR. C. T. JACKSON:

Dear Sir,—The subject of "Medical Geology," as it has been called, to which you have drawn the attention of the public by a letter published in the newspapers, is, undoubtedly, one of very great importance, and should not be overlooked in the surveys and reports made by the geologists under the authority of the General Government, and of the several States.

Your suggestion that there is a greater amount of cholera in limestone districts than in those based on granitic and other primary rocks, is now receiving melancholy proof and confirmation at Sandusky City, in Ohio.

In the course of your investigation of the geology of the West, you must have discovered places where, over considerable areas, the rocks are entirely bare of drift or soil of any kind. They appear to have been points over which the currents of the ancient ocean swept with unusual force; and upon which, consequently, no loose materials, no clay, sand, gravel, or boulders were deposited. Several such places are to be found in the region of the Upper Lakes. Sandusky City is one of them.

The "corniferous limestone" rock here forms the surface of the ground upon which the "City" is built. There is not soil enough upon it to support shade trees on the streets and public grounds. A lady of great taste and intelligence informed me that she was compelled to send into the interior for earth, with which to make a flower garden and shrubbery about her house.

Now, if the suggestion above referred to is correct, we should, *a priori*, expect a greater development of cholera at Sandusky City, than at other places, both as to its amount and malignancy; and the facts are precisely in accordance with this view.

It commenced suddenly, and with great severity, on the 24th of July, there having been thirty-six deaths on that day. A great panic immediately prevailed, and a large proportion of the inhabitants fled. Business was almost entirely suspended. When we reflect that the population of Sandusky City is only about twenty-five hundred, this

will be considered an unusual degree of mortality. The same proportion in the city of New York, would carry off six thousand persons daily. From that time to the present, the disease has continued with but slight abatement of its severity, the number of interments varying from twelve to thirty-three daily. The physicians of the place became exhausted and worn out, by their constant attendance on their patients; and several others from Cincinnati, Cleveland, and places in the interior of the State, have, with great humanity and heroic courage, come to their assistance. Dr. Ackerly, from Cleveland, writes that he is convinced that the cholera at Sandusky City, is of a more severe and malignant form than at other places in the country. "In some portions of the town," he continues, "inhabited by the German and Irish population, the disease has become infectious, a cholera atmosphere, (or stench,) can be observed along the entire length of some streets, but more particularly in the vicinity of houses where there are many sick and dead."

I am induced, in view of the great importance of this new branch of Medico-Geological inquiry, to address you this letter, in the hope that the facts herein set forth, may induce men of science to give it their attention in their future investigations in these subjects.

Very respectfully yours,

I. A. Lapham.

Dr. Jackson asked Prof. Agassiz if he could not secure investigations, by testing the atmosphere where the cholera prevails, to see whether there was any peculiar prevalence of animalculæ.

Prof. Agassiz replied that he had no doubt there was an immense atmosphere of life surrounding us. This had been discovered on the heights of the Alps, covering the snow with the appearance of blood. He understood that in Boston harbor the waters were green, as he supposed from animalculæ; and he understood the fishes had disappeared.

Dr. J. C. Warren said, it was a well known fact that cholera prevailed in moist situations, where there was, or might be, a decomposition of animal and vegetable substances. This was the reason, no doubt, that it followed the courses of rivers and marshes, in so many instances. In fact, it affected much the same situations, and pursued much the same course, as intermittent fevers. He proceeded to state facts to corroborate this. As to the non-prevalence of cholera in granitic regions, he had not been able to discover evidence sufficient

to support this opinion. Granitic regions were generally dry, and, of course, not so likely as low and marshy places to be the seats of miasmata, produced by animal and vegetable decomposition. To prove that cholera did not appear particularly in granitic regions, required a great number of facts carefully observed and clearly expressed. In the vast regions of Russia, Prussia, and other countries of Europe, where cholera has very extensively prevailed, where it has been very carefully watched, and where the results have been published to the world by acute and able observers, it has not been averred that cholera was excluded from granitic regions. He recollected one striking fact in opposition to this doctrine, where the cholera occurred in a granitic region, in the interior of India, with a malignity altogether peculiar.

Dr. Jackson inquired whether, in this instance, the waters had not been drawn from calcareous regions. But he did not limit the disease to these districts. There was an extensive choleric predisposition, and there are many exciting causes, among which he believed the use of calcareous waters was an important one.

Dr. J. C. Warren said the most philosophical opinion of the origin of cholera was, as it appeared to him, that the predisposing cause to the disease, was a poison diffused through the atmosphere, which rendered the human body liable to be affected by it on the application of an exciting cause. This cause might be indigestible food, too much food, calcareous waters, &c. These produced a violent commotion, in which nature made efforts to throw off the poisonous substance, and, in fact, succeeded in doing so, in the greater number of instances. To show the probability that cholera was an atmospheric poison, he would refer to the fact that the premonitory cholera, so called, pervaded this country from one extremity to the other, during the past year — that is, in the summer of 1848. A disease so extensively diffused, could not be conveyed by any agent we are acquainted with, excepting the circumambient air. As to the connection of the premonitory cholera of 1848, with the malignant disease which has existed so extensively in 1849, while it may be impossible to prove it, most persons will allow it to be highly probable.

Dr. Jackson said he had no doubt that the cholera prevailed most in limestone regions.

The Secretary read the following communication:

ON A PROCESS FOR DETECTING THE REMAINS OF INFUSORIA, &c., IN SEDIMENTARY DEPOSITS. BY J. W. BAILEY, Professor of Chemistry, U. S. Military Academy, West Point.

IT often happens that in the sedimentary deposits containing siliceous shells of *Infusoria*, and other microscopic organisms, the inorganic matters with which the minute forms are mingled, bear a large proportion to the organic bodies, and correspond to them so nearly in specific gravity, that it is difficult to detect their presence. In such cases, the following mode of examination will be found to yield very satisfactory results.

Thoroughly *dry* the materials to be examined, the minute shells which are unbroken, will then become filled with air. If in this condition, a portion of the sediment be rapidly stirred up in water, *the shells will rise to the surface*, each being *buoyed up by the air temporarily enclosed.* If the water is then allowed to stand undisturbed for a few seconds, the sand and mud, which may have risen to the top, will subside, leaving the organisms floating by themselves. These may then be easily removed from the surface and transferred to glass by simply touching the finger to the surface of the water, and then to the slide on which they are to be placed. The sediment, if dried again, will often yield another abundant supply of the minute shells.

By the above means, I have obtained exquisite specimens from the bottom of dried up ponds, from the sands of harbors, and from the mud attached to floating ice in the Hudson river — materials presenting the two extremes of very coarse gravel and the finest sediment, neither of which would have given good results by any other process.

Prof. AGASSIZ said that the method employed by Prof. BAILEY to procure specimens, was an excellent one, and was also the method employed by d' Orbigny.

ON THE EMBRYOLOGY OF CEPHALOPODA. BY PROF L. AGASSIZ.

[Not received.]

A MONOGRAPH OF THE FRESH WATER COTTUS OF NORTH AMERICA. BY CHARLES GIRARD.

A monographic investigation of the fresh water Cotti of this country, has led me to very different results from those to which ichthyologists have heretofore arrived.

In spite of the apparent uniformity which characterizes the genus *Cottus* of authors, I believe it necessary to subdivide it by placing on one side the marine species, and on the other the species of fresh water.

The primitive type of the genus being the *Cottus gobio* of Europe, it will appear at once very natural that the name of Cottus should be retained for the analogous species.

I therefore propose the generic name of ACANTHOCOTTUS, for the marine species.

Agreeing with Mr. Heckel in the propriety of withdrawing the *C. asper* of Dr. Richardson from the genus *Cottus*, I however do not coincide in the opinion of this naturalist with regard to its affinity with his *Trachidermis fasciatus*.

In the Ichthyology of the "Sulphur," Dr. Richardson has established the genus *Centrodermichthys* to include some fishes of the China seas. He believes that his *Cottus asper* will find there its place. Not being able to decide this question for the present, I shall take it up again before the publication of my monograph.

With regard to the true genus *Cottus*, one will recollect that the species have been united into one species, and have been regarded as identical with *C. gobio* of Europe.

But even in Europe there are not less than four species well described, (*Cottus gobio, affinis, pœcilopus*, and *microstomus*,) and I have pointed out some others hitherto confounded with the *gobio*, to be comparatively studied and described.

Asia seems also to possess its own species, but they are as yet very imperfectly known.

I know not less than ten American species, many of which are new, and all are distinct from those of Europe. They are :

COTTUS COGNATUS *Richards.* Faun. Bor. Amer. III. 1836, p. 40. Great Bear Lake ; Dr. Richardson.

COTTUS RICHARDSONII *Agass.* Lake Sup., 1850.—Northern shore of Lake Superior ; Prof. Agassiz.

COTTUS BAIRDII Grd.—*Cottus gobio* Kirtl. Bost. Jour. Nat. Hist. v. 1847, p. 342.—Body subcylindrical, short ; mouth comparatively large.—Pennsylvania, tributaries of the Ohio ; Prof. Baird. —Mohoning River ; T. P. Kirtland.

COTTUS MERIDIONALIS Grd.—Resembles the former, but the tail is tapering away more rapidly. The mouth is also a little larger. Enter James River ; Prof. Baird.

COTTUS FRANKLINII *Agass.* Lake Super. 1850. — Eastern and Southern shores of Lake Superior; Prof. Agassiz.

COTTUS VISCOSUS *Hald.* Suppl. to a Monogr. of Limn. &c., 1840, p. 3. — Eastern Pennsylvania; Prof. Haldeman, Baird.

COTTUS GRACILIS *Heck.* Ann. d. Wien. Mus. II. 1837, p. —. *Uranidea quiescens* DeKay. New York Fauna, 1842, p. 61. Pl. V. fig. 14. — *Cottus gobio* Ayres, Bost. Jour. Nat. Hist. V. 1845, p. 121; Pl. XI. — New York; Heckel, DeKay. — Manchester, Conn.; O. W. Ayres. — Massachusetts; Dr. Storer.

COTTUS BOLEOIDES Grd. Remarkable by its slender body and its fins much developed. — Windsor, Vt.; Ed. Cabot, Dr. Storer.

COTTUS GOBIOIDES Grd. Body thick and short; mouth very large. Burlington, Vt.; Rev. Z. Thompson, Dr. Storer.

COTTUS FABRICII Grd. — *Cottus gobio* Fabr. Faun. Grœl., 1780, p. 159. Greenland; Othon Fabricius.

THE following oral communication was presented at the General Meeting in the evening of yesterday: —

ON ANIMAL MORPHOLOGY. BY PROF. LOUIS AGASSIZ.

Gentlemen of the Association: — Before I proceed to illustrate the subject which has been chosen for this address, I propose to define what is meant by the morphology of the animal kingdom. In the progress of zoölogical investigation, during the study of the structure of animals, and their mode of growth, questions have arisen which require further investigations than those to which naturalists have been devoted. Up to the present time, it is very well known, the objects of the investigations of zoölogists have been chiefly to study the internal structure of animals; to determine their true relations to each other; to inquire how their different functions are performed; to ascertain how they grow; to compare the extinct species with those now living; in fine, to arrive at an insight into their different relations, whether structural, physiological, or geological. But the results of those investigations, up to the present time, have led only to classifications, — to theories. I think there is a higher aim in science than mere classification, than mere theory, however wide may be the conclusions derived from those investigations. It is my opinion that we are to seek the recognition of the plan according to which animals have

been created; and I think that this object is widely different, both from classification and theory. Classification may be simply an ingenious way to account for what we see, entirely derived from the efforts of our own intellect, and sufficiently elaborate to be satisfactory to our minds; but the question which we have to settle, in my judgment, is whether there is really in creation a plan which does not result merely from our contrivances to illustrate the subject before us, but which is inherent in nature; whether the animal kingdom is constructed upon such a plan independent of our recognition of it; and whether our recognition of the plan will not carry us to the recognition of the cause and origin of the animal kingdom itself.

By study, we have approached towards the knowledge of the plan displayed in the animal kingdom. We have been able to trace many relations which could not be ascertained at first sight. We have discovered relations which remained unintelligible until extensive comparisons had been made, giving us an insight into them. We have raised the question, whether there is not really in the animal kingdom, a plan illustrating the principle of its origin. To express it in one word, we have been led to consider the animal kingdom not merely as a beautiful combination of isolated phenomena, easily brought into intelligible connection by the efforts of our minds, but as a *Thought of a Supreme Intelligence manifested in material reality*. That is the view I take of the animal kingdom; a view which greatly differs from the one generally entertained regarding it. Is it possible to reconcile what we know at present of the animal kingdom with the idea that in all its diversity, it has sprung up, as it were, from inorganic nature, has been developed upon the surface of our earth, and now presents this gradual unfolding into three various yet analogous forms, upon a plan intelligible to man, and all this simply owing to its real existence, and to the fact that man is an intelligent being? Or do the relations which exist in nature show satisfactorily that all classes of animals and all individual animals are partial expressions of a general thought and manifestations in material reality, of a plan laid out by a Supreme Intelligence?

It is next my object to describe the manner in which we should ascertain the probable correctness of one or the other of these views. The first point of the investigation would be to examine at once whether it can be shown that it is a correct view of nature to consider it as the manifestation of the thoughts of the Creator. Again, we should have a strong inference that the animal kingdom is simply the

result of natural causes, if it could be shown that animals of various kinds are evolved from each other, under different circumstances. But extensive as have been the investigations of geologists, there has not been, up to the present day, a single instance discovered from which it could be shown or inferred that the various kinds of animals which are found in the different strata composing the crust of our globe, were derived from those existing at a prior epoch. On the contrary, in every one of these successive beds, we find different kinds of animals, bearing, no doubt, some relation to each other, and sometimes so much resembling each other in the succession of time, or in the changes which they undergo, from the germ to the full grown individual, that we are very apt to consider this succession of individual types as a natural reproduction of the changes which the full grown animal undergoes. But to my mind, this impression seems a mistake. We perceive by our intelligence, the connection between all these animals, and because this connection is in accordance with the law of our thoughts, we readily infer that it is the cause of the existence of beings, between which, after all, we are unable to discover any material connection. In order that this material connection should be considered as an evidence of a growth of the latter forms from the earlier ones, it were necessary that some later group of animals should be traced back to an older type, as its ancestor, which has never been done.

There are periods in the history of our globe when fishes are the only type of the vertebrated animals. There is a later period, when reptiles are added to the fishes. There is a still later period, when birds and mammalia are added to the reptiles and fishes. Now, though the earliest fishes are not much like the fishes of the present day; though the earliest reptiles have characteristics which we find only in fishes at present; though the earliest reptiles have characteristics which remind us of the birds and mammalia which follow at a later epoch; there is not one case from which we can infer that the first reptile which existed was born from a fish, or that the first mammal which existed was produced by a reptile; which should be the case, if we were to admit such an evolution of the different later types out of the earlier ones. Therefore, I conclude that it is a mistaken impression of our minds, that it is a misapprehension of the operation of our intelligence, when the apparent connection which exists in the order of succession of all these beings is taken for a causal connection.

But there is another point of view in which we may consider these phenomena. Suppose, for a moment, that all that exists was produced by a Supreme Intelligence — I speak as a scientific man, allowed to take no evidence from other quarters than investigation, and therefore I must make this as a supposition, a merely possible case, — suppose that these beings originated in a Supreme Intelligence, and that the operations of our mind were in accordance, though in a much lower sphere, with the plans laid out by this Supreme Intelligence, how easily might this accordance and this harmony lead us to take what has been the result of a higher Intelligence, as a causal connection, to which we should assign the different developments! Have we not sometimes, in reading the works of an author, felt as if we had written what we read, when we only understood it? Have we not felt as if we originated thoughts which we receive from others? Shall we not then concede for a moment the possibility that the thoughts we derive from looking at the works of nature, may not be the expression of a causal connection between those phenomena, but merely an impression like that in which in the writings of another author we recognize the image of similar thoughts in our minds; and that, therefore, instead of allowing these phenomena to lead us to support the view of those who attribute to natural causes the production of the creation we see before us, we should recognize in the harmony that exists between the operations of our minds and the plan which we discover in nature, another evidence that we are made in the image of Him who created every material thing, and are endowed with the ability to understand the plan upon which He has formed these varied creations?

I have taken this as a supposition. Now let us see whether we have in nature any evidence that there is such an intellectual — not material — connection between the phenomena which we discover in the relations of the different animals throughout the animal kingdom.

Our investigations have allowed us to distinguish in the whole animal kingdom four types — *Vertebrata*, *Articulata*, *Mollusca*, and *Radiata*. These four types, anatomists have shown to differ in their structure, and to differ so widely that while there were attempts made to discover a gradual unique series throughout the whole animal creation, a simple series passing through all created beings, it has finally been found impossible to avoid the fundamental distinction between these four types, which are so distinct as not to form a natural gradation from the lower to the higher. Let me explain more precisely this result, by the following table:

Vertebrata.	*Articulata.*	*Mollusca.*	*Radiata.*
Mammalia,	Insects,	Cuttle-fishes,	Starfishes,
Birds,	Crustacea,	Snails,	Jelly-fishes,
Reptiles,	Worms.	Clams.	Polypi.
Fishes.			

Among the *Vertebrata* we have four classes; among the *Articulata*, three classes; among the *Mollusca*, three classes; and among the *Radiata*, three classes, of which I give here the common names instead of the Greek, to remind you at once of some of their representatives. Now, if there was a single series in this animal kingdom, without any break, we should find that the highest of the *Radiata* should resemble most the lowest of the *Mollusca;* that the highest of *Mollusca* should resemble the lowest of *Articulata;* and that the highest *Articulata* approached the lowest *Vertebrata*. Now, on the contrary, the fact is, that if we look for the most striking divergence between the types of these different groups, we shall always find them between the highest of a lower type and the lowest of the type next above. So that there is a plain evidence in the nature of the phenomena themselves that the animal kingdom cannot be considered as a series of successive developments. Starfishes and sea-urchins, are those *Radiata* which differ most from clams, the lowest of the *Mollusca*. What resemblance can there be between an oyster and a starfish? As little as we can imagine between two different beings, belonging to the same kingdom. What resemblance is there between a cuttle-fish and a worm? What resemblance is there between a butterfly, the highest type of insects, and a fish? It is plain that such a series, as the expression of the true relations between animals, cannot be acknowledged.

These four types are so distinct that all the results of modern investigations in embryology, in comparative anatomy, in the minute comparison of external forms, gradually lead us more and more to consider them as completely different, and to recognize in each a particular plan of structure, a plan which can be expressed under simple formula, so uniform is the structure of all those belonging to the same type, though in form they may differ widely. For the type of *Radiata*, we may put down, as representing their true organic character, the figure of a star. The details of their structure are all framed upon bases to which a star would be the most general expression. *Polypi* are radiated in their fringes. Jelly-fishes are radiated in their structural arrangement. And the most careless observer must perceive

that the starfish is radiated, as its name indicates, in every language, not only among civilized nations, but even among savages.

Mollusca have an entirely different structure. You perceive no radiated arrangement. The parts are symmetrical along the two sides of a longitudinal axis, with the anterior extremity distinct from the posterior extremity, from which we may distinguish the upper and lower regions. In the star-shaped type, we have no above or below, unless we assume that the opening of the mouth is either above or below, and then we are left to make an arbitrary decision as to which we shall consider it. The mode of development in *Mollusca* is such that we are able to express it in a simple figure which shall indicate their form by an oval, shut by the reunion of the two margins uniting below. I might enter into many embryonic and anatomical illustrations to show the correctness of this emblem, but I must either suppose that they are known to you, when it would be useless to repeat them, or that they are unknown, in which case what I might be able to state in several hours would not add any thing material to the evidence of what I have to say. I therefore rather take the facts as stated by physiologists and anatomists as granted, in order to trace the course of reasoning which I have marked out.

Among *Articulata*, we have also symmetrical forms. They have two sides, and distinct heads, an above and below; but the manner in which they are developed in the egg so differs from the development of *Mollusca*, that though we may express them both by a longitudinal line, yet to express the typical difference, we must make a distinction. They disagree, not in being symmetrical, but in the manner in which the symmetry is established. We may have a bilateral symmetrical arrangement connecting them all, but the body of *Articulata* is transversely divided into movable joints, and the germs unfold backwards. So among insects we have a mode of development which is the reverse of what we have in *Mollusca*, that is to say, the germinative membrane, instead of folding downwards and uniting at the lower surface of the animal, folds from below, upwards, and the two sides of the membrane unite upon the back. You have seen, no doubt, the skin of a locust, or the chrysalis of a butterfly, opening upon the back. This is exactly the reverse of what we observe in *Mollusca;* so here we have the emblem of the *Articulata* in an oval shut above by the reunion of its two primitive margins. We have emblems, as it were, to represent the different thoughts which lie at the bottom of these structures, or the plans upon which they are

made; and these plans, as you see, differ so much as to present no possibility of being reconciled one to another. At least, this is the impression of naturalists of the present day. It seems as if there was an absolute barrier between these different types, as if we had here gained an evidence of the deep foundation of these separate plans, a cause deeper than local circumstances and physical causes, as these plans are common to many different animals living everywhere together.

Vertebrata may also be expressed by an embryonic sign, in the form of an 8. I shall only state that this body is divided into two cavities, one above and one below; the lower containing the heart, the respiratory organs, and all those organs connected with the alimentary canal, which preside over the functions by which life is maintained; while the upper cavity contains the nervous system, the brain, and the spinal marrow. These organisms present a plan of structure entirely peculiar. There is here again no apparent connection between them and the other types. I have already shown you that *Articulata* differ from *Mollusca* in having articulations all along their bodies, instead of presenting a continuous undivided mass like slugs. The body of the *Articulata* is divided into rings; and besides this development of their external characters, there are additional differences exemplified in these types, as well as in the two others. We may, therefore, take the four emblematic signs just introduced, as representing the four great divisions of the animal kingdom; as representing the four different plans upon which they are constructed, as exemplified by their differences.

In this investigation I have one further step to trace: — Are really all these classes created upon their several plans — *Vertebrata* upon this, *Articulata* upon this, *Mollusca* upon this, and *Radiata* upon that — and is there no transition from one plan into another? It has been the object of naturalists for the last forty-two years — (it originated with Göthe, and was extended by Oken and others) — it has been the object of the continued efforts of all anatomists and physiologists, to trace the resemblance which exists between the diversified parts of the body of the animals belonging to these different types. Among the *Vertebrata*, the evidence has been completely conclusive. There is no one at all conversant with comparative anatomy who does not know that there is no doubt left as to the identity of the structure of fishes and mammalia. The human frame is constructed upon the same plan with that of fishes. There is in both a head, with

movable jaws in the face, which corresponds to each other so closely that, notwithstanding the difference of form, any individual bone in the one can be referred to its corresponding type in the other. We have backbones consisting of the same elements, and identical in their foundation, throughout the vertebrated type. If, then, the extremes so resemble each other, I need not point out the evidence that birds and reptiles agree also. The fins, pectoral and ventral, in fishes, correspond to the organs of locomotion in other Vertebrata; the ventral fins corresponding to the hind legs, and the pectoral fins to the fore legs, the arms, or the wings. The different forms which these organs assume are no more different to the eye of a trained anatomist, than if they were constructed of the same individual parts, and possessed the same external appearance. The wing of a bat, and the arm of a man, are the same apparatus, in the eye of every anatomist; and the resemblance is as plain to him, as any mathematical demonstration can show to the geometer the identity of figures which at first do not seem to have direct relation.

This evidence is generally acquired by tracing intermediate forms; by finding, for instance, animals with elongated arms, which present a form intermediate, between the wing and the common form of an arm, or the fin of a seal. Embryology has shown that the extremities of mammalia are all identical in their structure, and that the wing of a young bird, the fin of a young turtle, or the fore leg of a rabbit or squirrel, have the same identical form; and that the differences which they assume are only the results of later development in their growth. I have pointed to this one series of facts to show in what manner the identity of structure may be traced. And I will add that the identity of all the different systems of organs has been traced in several classes as fully as that of the solid bony frame, or the organs of locomotion. The brain of all *Vertebrata*, whether in man or fish, however different the size, however different the function performed by this organ, has the same structure. They only differ in the intrinsic development of the parts, and the amount of power with which they are endowed in accordance with their different development. The resemblance of all the parts may be traced from the lowest fish to the highest mammalia, man himself; and in all we have only modifications of form with the same arrangement of parts. Thus we may take any figure which represents this type, as a kind of unit upon which we may argue as we might with the whole series by enumerating successively all its members. Speaking of *Vertebrata*, therefore,

I shall speak of them as a unit. And so I may do in speaking of insects, crustacea, and worms; the different rings of which the bodies of worms are composed, the little appendages of stiff hairs, inserted upon these rings, are only the lower condition of the complicated apparatus existing in insects, where there are legs; where there are a distinct head, a distinct chest, and different parts of the body; and where in addition there are wings in greater or smaller number. Even all this complication can be reduced to one ideal unit, which is represented by the form I have already described.

It is plain that it is among the lowest *Articulata* that we find indications of a closer resemblance to the *Vertebrata*. There is scarcely any resemblance between the lowest fishes and the highest *Articulata*; but there is a real resemblance, in many respects, between the lowest fishes and the worms. We have evidence which shows that all types arise from a foundation where scarcely any difference is yet introduced, but with different modes of development; so that all these forms, which at the outset come so near together, diverge upon different plans, and tend to different ends.

Mollusca seem to differ so widely, that it is almost impossible to trace such a resemblance between their different members as we find among *Vertebrata;* and indeed no attempt to identify the various elements of their structure has been made of late. Nor has it been attempted among *Radiata*. The differences between the structure of starfishes and echini have even been misunderstood — both these families being highly complicated, presenting apparatus as widely different from each other, and as complicated in their adaptations, as the highest *Articulata*. This complication of structure has made it almost impossible to trace the correspondence which exists between these highest *Radiata* and the lowest *Polypi*. But having been induced to make these animals the subject of special investigations, I have traced these analogies, and have satisfied myself that there is as great a resemblance between the extremes, in these lower classes, as between the extremes in the higher classes; so that the whole range of *Mollusca* — cuttle-fishes, snails, and bi-valves, or clams — are constructed not only upon the same general plan, but their different parts correspond precisely. I will not go through the illustration of these facts, as I have already made that the subject for a communication to one of our Sections; but I will only repeat the conclusion at which I have arrived in these investigations, — that we can trace among *Mollusca* and *Radiata*, the same identity of the details in the arrange-

ment of the parts — the same identity of plan in which the development is carried out in the different classes of the type — as has been traced in *Articulata* and *Vertebrata*. So that we can arrive finally at this conclusion: that each type is really constructed upon a peculiar plan, and that all the members of each type, however diversified among themselves, can be reduced to one simple formula — to one simple expression.

I have attempted not only thus to represent these different types, as distinct plans, but to illustrate their unity. I would now ask whether there is no connection to be discovered between them — if we have arrived at the last conclusion of our investigations — to acknowledge four distinct plans of the animal creation, differing in their arrangement; or whether they are not to be reconciled to one thought; — whether there is an intellectual connection between these plans, showing that they are the realization of a pre-conceived, and hence intelligent and intended plan laid out before their creation, and carried out in reality, in a succession of types? If I succeed in showing that there is such an intelligent connection between these plans, then, I think, I shall have shown, on scientific grounds alone, that we are to bow before a Supreme Intelligence, and acknowledge in science what we feel so deeply in our bosoms.

How are we to consider the expression of the lowest type, (the star-shaped emblem of *Radiata*)? Let us look at it from an embryological point of view. I have alluded to the fact, that *Radiata* begin low in their series, with *Polypi*; *Mollusca* begin low in their series, with *Acephala*, or the oyster family; *Articulata* begin with worms; and *Vertebrata* begin, in their series, with fishes. There is little resemblance in the forms, or in the characteristics even, of the lowest representatives of all the types, in their full-grown condition — though they come nearer to each other than the higher ones of the same type. Now let us trace them prior to their full-grown condition — in the egg — and see what we can learn from embryonic evidence. I shall be compelled to be very short, and perhaps obscure on that account, for I shall be forced to make statements, instead of giving evidence; but I hope that in referring to material facts, I shall be trusted in my quotations.

One of these material facts, resulting from our embryonic investigations, is, that all animals, without exception, arise from eggs; and that all these eggs are identical; that they are, at the outset, spheres. But this is the plan of structure of every *Radiate* at the beginning. It

is the plan of structure of every *Mollusc*, of every *Articulat*, and of every *Vertebrate*. They all start from a uniform structure. Whatever, therefore, may be the difference in the full-grown species, they commence with a material unity, which is led into material difference in the process of time. Without illustrating the changes which *Radiata* undergo, we have finally, in the full-grown animal, the form of the *Polypi*, the *Medusæ*, or the *Echinoderms*, where we had originally a simple egg. This egg, though at first a simple spherical cell, of various size, — as in the bird's egg, or the egg of the tortoise, and even of mammalia, — produces *Radiata* of all types, according to their species. Therefore, we see that a simple egg may grow into a large material being, with all the apparatus necessary to sustain life. So we may say that all *Radiata* are living eggs, with an opening to establish a communication with the surrounding world. This opening may be placed downwards, or may be placed upwards. The animal may move in all directions — may even move with the opening forwards — but nevertheless, in every instance, preserves its spherical form.

The *Mollusca* seem to differ widely, since when full-grown they have a longitudinal axis. But the elongation arises from the development of a sphere. Conceive a sphere, in the type of *Radiata*, to be elongated in the direction of the opening, and you have the regular sphere of the *Radiata*, passing into the oval-shaped *Mollusca*. The snail is an elongated sphere, the opening of which is at one end, and the body of which is split below. The split remains open in the oyster. The two sides of the split, from the mouths of the shell of the oyster being raised and somewhat reduced, will form the walls of the mantle and shell of the snail, which creeps upon the muscular part which remains concealed between the valves of the oyster. Then the anterior extremity of the oyster, being surrounded by peculiar appendages, and growing somewhat larger, will become a head, which may move forwards as in the snail, in which the progressive impulse given in the lowest condition of the oyster, is still made more prominent.

And here we have the first indication of the sphere passing into the elongated form; the radiated type passing into the type of the *Mollusca*, though in their full-grown condition, there is not a possibility of reconciling one with the other. I will not go farther into the illustration of the facts. I can only say, that among *Articulata* the split is upon the back, and as the parts develop the folds bend backwards; that there are transverse articulations formed besides the main longi-

tudinal cavity. In *Vertebrata* the inner division is two-fold. Now, these differences are progressive steps in the development. The connection which we thus perceive is not material; it is perceived by an argumentation, and nevertheless it is real. But it is perceived because we go beyond the form to the development of the idea which was the cause of the form. It is the ideal image which we perceive; it is the process which is before you. You see the idea developing, as when you observe the artist before his canvas, upon which there is as yet nothing, but upon which soon appears the idea of his mind. The connection between these lower creations, in their development from an identical form, upon different types of structure, shows that it is the plan of an intelligent being, capable of understanding it, and of referring each to its true place. If it were not so, why should not all these eggs continue in the same state; or why should the same causes produce from the egg such a variety of animals? In the same sheet of water, the egg of the *Radiata*, of the *Mollusca*, and of *Fishes*, are placed under the same circumstances. If they were not primitively endowed with an immaterial power of developing materially various forms, why should they not go on in the same unity of structure? We have therefore evidence, as plain as can be, that there is an immaterial principle in every one of these germs, which is the reflection of a part of the whole plan, and the connections of which are so complicated as to hint constantly from one part to another, as the connections which exist between these animals are not only structural and genetic, but are also successive in time, as I shall now presently show.

These different growths, these different modifications in the forms, typify as it were, the successive changes which have taken place at different periods in each complicated structure. Geology has taught us that there is a succession of different types; that in the earliest period of the creation or development of our globe, only certain of these beings were created; at a later period, others were introduced; and at a still later period, still others; and so on. Now what is the order of this succession? It agrees exactly in all its details with the gradation which we observe when we trace simply the structure of embryonic forms; so that the order of succession is only another form for the expression of the same thought.

We find in the oldest representative of any of these types, the condition of the earlier embryonic states of any of those living at present, which belong to a higher family of the same type. Whether

we look among the *Vertebrata*, or among the *Articulata*, or the *Mollusca*, or the *Radiata*, it can be shown by evidence derived from palæontology and geology, that the older are, as it were, the embryonic forms of those which live in our days; that, for instance, the first *Crustacea* which existed, resembled images, grown to full size, of the first stages of development which we observe in the egg of our lobsters or crabs; that the earliest starfishes which existed, were the living representatives of the earliest microscopic forms which we observe in our starfishes when they are forming within the egg. The egg or reproductive cell is, therefore, in every point of view, morphologically, anatomically, embryologically, or geologically, the organic unit of the animal kingdom.

But in the succession of organized beings we find such a progress, that tracing all these relations we arrive at man as the last. He is by his structure the highest. He is in the order of succession the last. And as we have traced all these different connections with reference to the plan laid out at the beginning, at what conclusion do we arrive in the most direct manner? It is that the creation of man was the aim of the plan from the beginning. And in a higher view, and without any reference to utilitarian considerations, we may say that this world has been made for man; for man was the object which the Creator had in view when he framed the plan for the development of this globe. And if this be the case, let us never forget what a responsibility it throws upon us, to be the object of such a development, and the close of such a magnificent construction as that which we have before our eyes every day; and let that be the fullest evidence that man was created in the image of his God.

On some Points of Linguistic Ethnology: with Illustrations, chiefly from the Aboriginal Languages of America. By Prof. S. S. Haldeman, A. M.

The classification of the elements is of great importance in the study of language, and I am convinced that a distribution of the consonants into *contacts*, as proposed in a rude way by the Abbé Sicard, is the only proper mode. These are essentially five, the labial, dental, palatal, guttural, and glottal. There are, however, some intermediate ones, or sub-contacts, which raise the number to *ten*, each

containing in general eight consonants, but as this number *may* be doubled, the entire number of consonants, in case all the blanks were filled, would amount to 160, or even to 200, if the cerebral and trilled varieties were included.

The latest work on the sounds employed in speech is, The Essentials of Phonetics, by A. J. Ellis, A. B., founded upon researches in the chief languages of Europe, which, although prepared with judgment, is clothed in an alphabet full of corruptions; primarily adapted to English alone, and being intended to replace the ordinary one, the most unjustifiable concessions were made to the present spelling of English. The common sense of Europe, Polynesia, Africa, and a great part of America, as well as those to whom these literary husks are offered, if made acquainted with the merits of the case, would reject them. And yet uniformity is to be found somewhere, because the Sandwich Islander spells the name of one of his islands *Maui*, and an English or American missionary, a Spaniard, Portuguese, Italian, German, Choctaw, or African Mandingo, would do the same. The excuse that the power of the *Latin* alphabet is "uncertain," (p. 222) is neutralized by his own opinion that the Latin vowel characters had their Italian and German alphabetic power, and we find an English author making an adjective HIBERIANA out of the English name *Heber;* and a German HYBNERUS out of the German name *Huebner*. Mr. Ellis cites the forms MAXIMUS and MAXUMUS to prove that U had "undoubtedly several sounds in Latin." He should have informed his unclassical readers that in these words, according to the ancient grammarians, the I and U *had not their true power*, but an allied one for which Claudius proposed a special character.

There is a gradual deterioration in the various alphabets of Mr. Ellis, the last being the worst. The analogy between the vowels in *meal mill; dale dell;* was recognized in 1844; but now, the last words are spelt "dal" "del;" and A, made for the sound in *far*, is perverted to the rare Welsh vowel in *fat*. Having reached its lowest point of deterioration, this alphabet is "brought to a satisfactory conclusion," and fault is found with those who will not adopt what are facetiously termed the "improvements." Yet his alphabet is still imperfect, his ideas of the open and close vowels being very confused. He does not properly distinguish the vowels in *paw*, *fur*, *pool*, *lo*, from those in *nought*, *worth*, *full*, *obey*, and sacrifices much to phonography (which has nothing to do with the printing of speech) leaving phonography itself imperfect, as it cannot represent the Eng-

lish *wh* as employed in some languages before a consonant, as in the Delaware *wh-t-e;* or the German j (English y) when *glauben* takes the dialectic form *jlaubn*. Mr. Ellis places the vowel of *fall* in *water*, *altho*, *short*, CROSS; and that in *not*, in *quarter*, *god*, *hog*, *horse*, CROSS. The inconsistency is obvious which demands a *different* vowel in *for* and *not;* and an identical one in *fur*, *nut;* a different one in *conclude* and *good*, but the same one in *endure*, *duty*, or (theoretically) in *coil*, *quoit*.

Labial consonants are rare in some of the American languages, the Cherokee having only *w* and *m*. The so-called whistle in Le-na-pe is merely English *wh* preceding a consonant, as in *wh-t-e*. No one has hitherto analyzed this sound, or any of the allied ones, which follow a law not yet announced; namely, that a whispered aspirate has a tendency to be followed by the same sound in a vocal and ordinary condition. The English word *when* is therefore wh-w-e-n; the sneering exclamation written h'm is (using a notation allied to the English) mh-m; the Welsh aspirate *l* is both lh-l and *lh;* so that Mr. Ellis has failed to hear an ordinary *l*. N takes the same phase in Cherokee, which has also an aspirate of the German J.

I have found whispered vowels, and even syllables, not uncommon in several American languages, as in the two final syllables of the Camanche word for *ten*, SEWANᴗhut, the first vowel as in *send*, but nasal.

The Wyandot language has many nasals, as in the word for *bear*, which in German characters (letting Italic *n* follow the nasal letters) is DANJ*n*O*n*J*n*E*n*>—, affording an example of a nasal liquid. The character > indicates a close of the glottis, which also occurs medial, as in my name in this language — HARRDA>AJE*hau*, the last syllable being nasal.

The Weko (Wako) has a medial or final clack or smack formed by the sudden separation of the closed fauces, independent of any action of the lungs, as in the word for *eye* — kitik⍖‘. The Nadako has an allied independent dental sound, which is more dull and less loud than the corresponding Hottentot clack.

In French, the nature of syllables is better understood than in German or English. The English word *luck*, is properly a dissyllable, but I have heard it as a monosyllable (meaning *six*) in Chinese (luck',) where it is pronounced without opening the organs at the close, for the escape of a faint aspirate. The same thing takes place with *p* and *t*.

Prof. HALDEMAN illustrated the above paper by examples from Indian and other languages containing the various glottals, clacks, and whispered syllables, to the great amusement of the audience.

Adjourned.

W. I. BURNETT, *Secretary.*

Seventh Day, August 21, 1849.

AFTERNOON GENERAL SESSION.

ON RUTILATED QUARTZ CRYSTALS FROM VERMONT, AND PHENOMENA CONNECTED WITH THEM. BY FRANCIS ALGER.

Mr. ALGER presented a paper on the quartz crystals from Waterbury, Vermont, containing acicular or capillary Rutile, and exhibited illustrative specimens of great perfection and beauty. He compared them with other specimens from the Alps and Brazil, and pointed out some important phenomena in which they differed from those, and all other rock crystals he had ever seen.

Erratic masses of rutilated quartz had, from time to time, been found in Waterbury, and several of the neighboring towns, and they had even been picked up in New Hampshire; but their geological association, or the character of the rock from which they originated, had not been well understood until recently. Mr. Alger had lately visited a remarkable locality of this mineral, where a true vein, two feet or more in width, had been brought to light in making a deep cut through a hill in Waterbury, on the line of the Vermont Central Railroad. The rock is a very tenacious talcose slate, sometimes passing into mica slate, and prevails to a great extent in this part of Vermont. Metaliferous minerals are rarely contained in it, but veins of quartz are common. The vein here referred to, consisted principally of common amorphous quartz, presenting internal cavities or druses, lined or studded with projecting prismatic crystals, sometimes colorless and transparent, but more frequently of a smoky color, or brownish yellow tint, (Caringorm.) The pure glassy white crystals, are but rarely penetrated by the acicular rutile, while the colored varieties abound with it, and seem in fact to owe the intensity of their color to the very prevalence of it through their substance.

The rutile is sometimes grouped in tufts of radiating crystals, proceeding from a common point, and shooting through the quartz; this being also the ordinary manner of its occurrence in the Brazilian specimens. The direction of many of these diffused crystals in the position they now occupy, would seem to show that they had been

subjected to some electrical or polarizing influence, by which they had been arranged very nearly in a line parallel with that of the apex or perpendicular axis of the crystals of quartz in which they are imbedded. It would seem that they were once floating, as it were, in the transparent and liquid medium of the silicious mass; or else, what is more probable, in the simultaneous crystallization of both quartz and rutile, slowly or otherwise, there was superadded a polarizing influence which caused them to converge towards one point. Or again, it may be that these peculiarities are confined to those quartz crystals which projected downwards in the cavity of the vein at the time of their formation; and thus the rutile, from its greater specific gravity, would have a tendency to crystallize and extend itself downwards, rather than in any other direction. Mr. Alger could not state from actual observation at the locality, whether such was the fact. The appearance referred to is the most marked in those crystals in which the rutile exists in the most delicate hair-like and needle-shaped forms, (*Venus hairstone;*) and, in some instances, these delicate prisms are bent towards the ends most remote from the apex of the quartz crystal. They are sometimes four inches in length.* By transmitted light, their color is reddish brown; lustre like that of polished copper. Some few of the needles are entirely black, and closely resemble schorl. It was the opinion of Mr. Kennedy, a scientific engineer, and a very close observer, who was present at the opening of the vein, that the crystals of quartz enclosing rutile, were confined to one side only of the vein, thus indicating two periods in its formation, in one of which no rutile was present to intercrystallize with the mass. All the recently obtained crystals are very much discolored by iron rust, and the vein appears to be "run out." But its loss will undoubtedly be soon supplied by other sources.

Prof. Hubbard, of Dartmouth College, in whose possession is the finest specimen of this mineral found in the United States, first noticed a most interesting fact in regard to these crystals, namely, that the needles of rutile in some cases, had shot completely through the quartz crystals, and stood out in relief upon their surfaces, as if protruded by the sudden effort of their crystallization. The same appearances were presented by one of Mr. Alger's specimens, to a small extent. If produced in the manner supposed, the quartz must have been in a liquid state; if not produced in that manner, the crystallization of

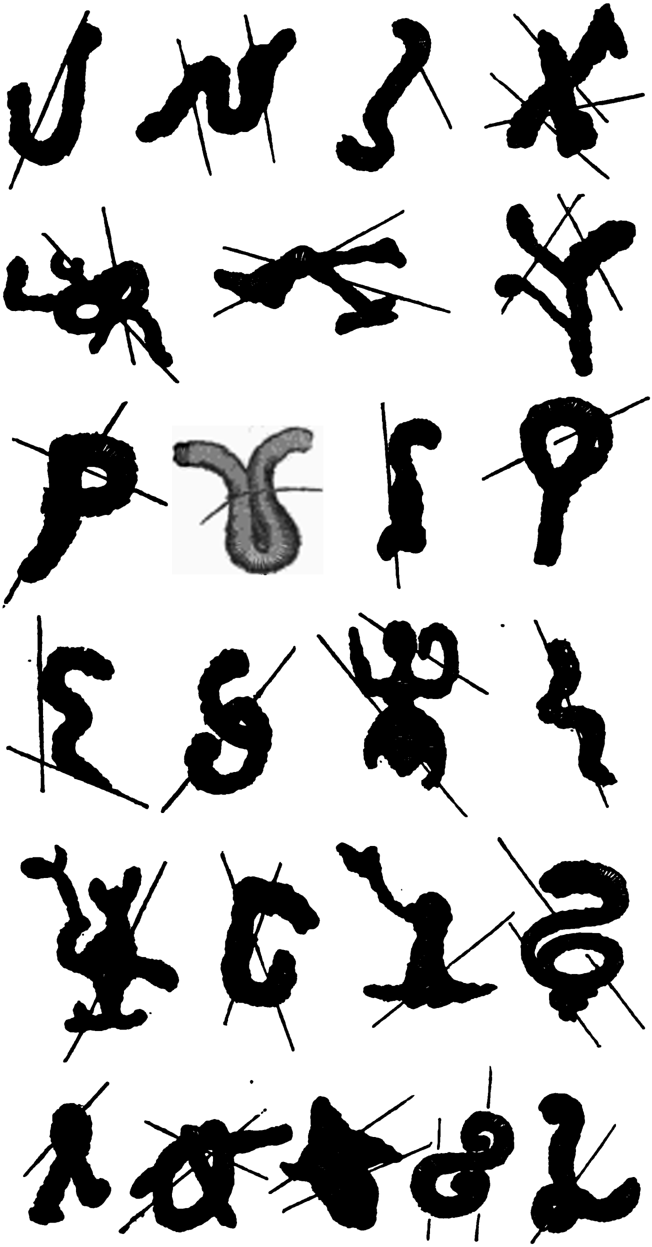
* The polished specimens in which these prisms are exhibited, (known in French jewelry as *Flèches d'amour*) are rarely surpassed in beauty by the finest to be met with in foreign collections. The reticulated forms are thus shown in great perfection.

the rutile must have continued after that of the quartz had ceased. The latter seems the most reasonable supposition, and is favored by analogous phenomena in other crystallized minerals.

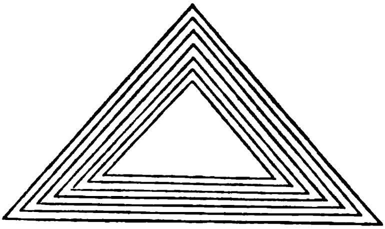
IMITATIVE FORMS OF MICA CONTAINED IN THE QUARTZ.

The surfaces of two of the large crystals exhibited by Mr. Alger, as well as several smaller fragments of crystals, were covered by minute but very brilliant scales of gold-colored mica; and these sometimes penetrated the quartz in company with the rutile, and, in the same manner, seemed confined mostly to the darkest colored varieties of the quartz. But the forms assumed by this mica, are curious and altogether unique, for in the substance of the crystals, it has assumed the most fantastical forms, appearing in tortuous and vermicular ramifications, some of them bearing such a striking resemblance to organized bodies, as to give the first impression that they are actually the remains of insects or worms. The figures on the next page present a correct representation in a magnified form, of some of the most curious of these appearances exhibited by Mr. Alger's specimens. He had dissected out several of them, and found them to be composed entirely of small plates of mica more or less closely united parallel with the basal cleavage of the mineral. In fact, they are elongated hexhahedral crystals of mica, twisted or distorted into every imaginable shape. Their laminated micaceous structure is shown perfectly by the microscope, and is represented in the figures. The resemblance of the third figure on the fourth row, to some species of Araneides, is not too remote to suggest them to the mind instantly; and the general resemblance of several of the figures to the common blood leech, (*Sanguisuga medicinalis*,) is still more striking. But the origin of these resemblances was evidently fortuitous, and could not have been in any way connected with organic matter. They are interesting principally as furnishing a new fact in the department of imitative mineralogy, and they appropriately suggest the term vermiform mica as most characteristic of their general appearance.

The straight lines seen passing through several of these figures, are intended to show the needles of rutile that actually intersect these concretions of mica in the body of the stone. In some of them the rutile passes through the circular space left by the folding over of the mica, and its crystallization does not seem to be interrupted by the mica in any case. A characteristic feature of rutile, but never shown in any of the specimens from this locality, (i. e. the geniculated forms,) seems to be somewhat assumed by the mica, and



is best shown by the second figure in the lower row.* The color of this mica by transmitted light, is a pale green, and the mineral seems to agree in external characters with the substance from other localities. Considerable quantity of it was found loose in the vein, mixed with broken crystals of rutile.† The only appearances at all analogous to those just described, which had come to the knowledge of Mr. Alger, were those mentioned and figured by Dr. McCulloch, and described in Vol. II. of the Geological Transactions of London. But in this case, the substance was chalcedony, and the imbedded masses composed of chlorite, had nothing of a crystalline structure, and in fact were rather imitative of vegetable, arborescent forms.



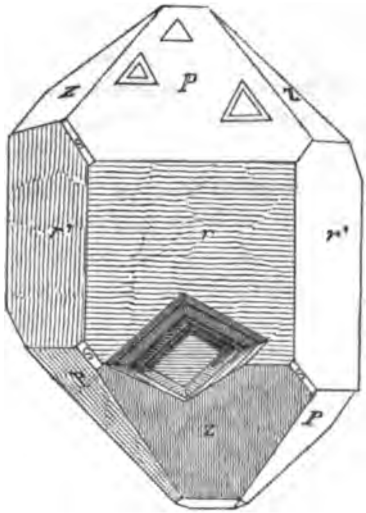
STRIATED QUARTZ CRYSTALS.

The above figure represents some of the striæ which appear on the acuminating planes of the crystals, and are parallel with their edges of combination with the adjoining planes, as shown in the figure on the next page. They are usually mere superficial triangular lines so slightly impressed as to be visible only when held in a particular position in regard to the light; but in a few cases, these configurations, commencing at a small point before the crystal had attained its full size, continue to widen with every fresh layer of particles deposited upon the faces of the crystal, until they produce cavities of considerable depth. That they were formed in this manner, is indicated by the step-like appearance of the sides of these cavities — an appearance which is more strikingly presented in a few cavities of a different

* Prof. Hubbard's specimen presented the appearance in so marked a manner, as to lead to the impression they were rutile.

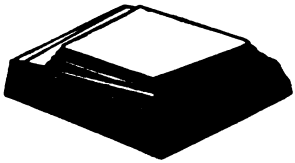
† A portion of this carefully separated, was found to lose nearly 15 per cent. of water when heated to the melting point of glass. A peculiar empyreumatic odor was at the same time given out, but there was no reaction of fluorine. Exposed in a platinum crucible to a white heat for twenty minutes, it became grayish black and partially fused into a mass. In this state it was slightly magnetic. The large proportion of water seems to ally it with ripidolite, or perhaps with hydro-mica from the Alps—*Wasserglimmer* of M. Morin. It will be further investigated.

shape, of one of which the following figure furnishes an example. In this, it will be noticed, the cavity is rhomboidal, and might at



first be mistaken for the impression left by some foreign substance which had disappeared. No substance having such form, has been found attached to any of the crystals from this place, and although the angles at which the sides meet each other, (about 76° and 104°) are nearly those of calcspar, or carbonate of iron, it is evident from the enlargement of the cavity towards the surface of the crystal, or the hopper-like appearance assumed by it, that neither of these substances could have produced it. The quartz, while depositing itself around either of them, would have taken the exact form of either, precisely as we see such impress of their forms in other crystals of subsequent formation. So it is evident, that if any substance ever occupied the cavity, it must have received its form from the cavity, without communicating any to it; and thus a pseudomorphous crystal may have been produced in a manner somewhat different from usual. It is not easy to trace any relation between these last named cavities and the crystalline structure of the quartz; their sides are not parallel with any of the striæ as seen upon the faces of the crystals; they indicate an interruption in the process of crystallization, by some cause not easily explained. The cavity measures one inch on a side, and is one half

inch in depth. It is exactly represented by the two small figures below, drawn in full size from a cast of it. The crystal itself, *rhombifère* of Haüy, has the replacement of rhomboidal planes on the adjacent lateral



angles of the prism, and not, as is usually the case, on the alternate angles. It is permeated in every part by acicular rutile. It is possible that a comparison of other specimens, should any more be found, will result in showing an agreement between the sides of these cavities and the edges of the small rhombic replacements lettered *s*. In this crystal, owing perhaps to some distortion, there is not so close an approximation to parallelism between the edges of the cavity and of these replacements, as would otherwise have obtained. No such cavity as this had been described before, and if the view here taken of it be correct, we have only to suppose a successive retrocession or withdrawal of particles in such parallel directions, thus enlarging the cavity outwardly, as the crystal itself increased in size. Something of the same kind is observed in artificial saline crystals. Several smaller cavities of the same form are presented by other crystals from the same locality, in some of which, delicate needles of rutile pass through the cavity unbroken — their extremities deeply penetrating the quartz.

ON THE APPLICATION OF THE PRINCIPLES OF ACOUSTICS TO THE CONSTRUCTION OF LECTURE-ROOMS, &C. BY PROF. HENRY.

[Not received.]

This communication was followed by remarks from President EVERETT and President SPARKS.

ON THE DIFFERENCES BETWEEN PROGRESSIVE, EMBRYONIC, AND PROPHETIC TYPES IN THE SUCCESSION OF ORGANIZED BEINGS THROUGH THE WHOLE RANGE OF GEOLOGICAL TIMES.

IT was a great improvement in our zoölogical investigations when the differences in their relations, according to the various degrees of

affinity or analogy, which exist between animals were pointed out, and successively better understood. In earlier times, zoölogists made no distinction between the different relations which existed among animals. Affinity and analogy, so dissimilar in their essential characters, were constantly mistaken one for the other; and upon the peculiarities which struck the observer most at first sight, animals were brought together, sometimes upon the ground of true affinity; sometimes also, upon the ground of close analogy. And, though comparative anatomy did put the mistakes arising from such confusion right, by showing that external appearances were sometimes deceptive, and that a more intimate knowledge of internal structure was necessary fully to understand the real relations between animals, there remained, nevertheless, a degree of uncertainty in many cases, as long as the principles of affinities and of analogies were not fully distinguished. Every naturalist now knows that true relationship — affinity, — depends upon a unity in structure, however diversified the forms may be, under which their fundamental structure is displayed. For instance, the affinity of whales and the other mammalia, was not understood, before it was shown that, under the form of fishes, these animals had truly the same structure as the highest *Vertebrata*.

Again, the forms of *Cetacea* exemplify the analogy there is between whales and fishes. They are *related* to mammalia; they are *analogous* to fishes. They bear close affinity to the mammals which nurse their young with milk; they have rather close analogy to the gill-breathing fishes.

Since the fossil animals which have existed during former periods, upon the surface of our globe, and which have successively peopled the ocean and the dry land, have been more carefully studied than they were at the beginning of these investigations; since they are no longer considered as mere curiosities, but as the earlier representatives of an order of things which has been gradually and successively developed throughout the history of our globe, facts have been brought to light, which now require a very careful examination, and will lead to a more complete understanding of the various relations which exist between these extinct types, and those which still continue to live in our days. Upon close comparison of these facts, I have been led to distinguish two sorts of relations between the extinct animals, and those of our days, which seem to me to have been either overlooked or not sufficiently distinguished. Indeed, the general results derived

from Palæontological investigations, seem scarcely to have gone beyond showing that the animals of former ages are specifically and frequently, also, generically distinct from those of the present creation; and also to establish certain gradation between them, agreeing more or less with the degree of perfection which we recognize between the living animals according to their structure.

It is now pretty generally understood that fishes, which rank lowest among the *Vertebrata* have existed alone during the oldest periods; that the reptiles, which in the gradation of structure rank next above them, have followed at a later period, that still later the birds, which according to their anatomy, rank above reptiles, have next made their appearance and that mammalia, which stand highest, have been introduced last, and even among these, the lower families seem to have been more numerous, before the higher ones prevailed over them. Man at last has been created only after all other types had acquired their full development. These facts, which, in such generality are fully exemplified in every country in the order of succession of the different fossils characteristic of the various geological deposits, show plainly that a gradation really exists in this succession, and constitutes one of the most prominent characters of the development of the animal kingdom as a whole.

If we investigate, however, this gradation, and the order of succession of animals more closely, we cannot but be struck with the different relations which exist between the fossils and the living animals. Many extinct types have been pointed out as characteristic of different geological periods, which combine, as it were, peculiarities, which at present are found separately in different families of animals.

I may mention as such, the *Ichthyosaur*, with their fish-like vertebræ, their dolphin or porpoise-like general form and several special characters reminding us of their close relation to the Crocodilian reptiles; thus combining characters of different classes in the most extraordinary manner.

Again, the *Pterodactyli* in which reptilian characters are combined with peculiarities reminding us both of birds and bats.

Again, the large carnivorous fishes of the coal period, combining peculiarities of the *Saurians*, with true fish characters; and so on.

These relations are of an entirely different kind from those which I have pointed out between some of the older fossils and the early stage of growth of the living representatives of the same families. For instance, the fossil fishes with a heterocercal tail, found below the

New-Red Sandstone, down to the lowest deposits, remind us of the peculiar termination of the vertebral column in all fish-embryos of species living in the present period, to whatever family they may belong, indicating a similarity of structure in the oldest representatives of this class, with the earliest condition of the germs of those animals in our days.

Let us now examine whether we can properly understand the bearings of these relations, and the meaning of such differences.

In the first place, I have mentioned the gradual progress, which is observed in the succession of the different classes of *Vertebrata.* This progress is exemplified by a series of types which differ from each other, but which show, when arranged in a series, a gradation, which agrees, in general, with the structural gradation, which we may establish upon anatomical evidence. For instance, the Salamanders with their various forms rank below the tailless *Batrachians.*

And where we have a succession of these animals in the tertiary deposits as they occur in various parts of Europe, we may fairly say that the fossils form, in their succession a series of progressive types.

Another example may perhaps illustrate the point more fully. The *Orthocera* of the oldest periods precede the curved Lituites, which, in their turn are followed by the circumvolute Nautilus. Here, again, we have a natural gradation of a series of progressive types. Again, among *Crinoids*, we find in the older deposites a variety of species resting upon a stem, while free crinoids begin to appear only during the secondary deposit and prevail, in the present creation, over those attached to the soil. Here, again, we have a series of progressive types developed successively, which are apparently independent of each other and seem to bear no other relation to each other than that arising from the general character of the group to which they belong. Such types exemplify simply in the groups to which they belong, a real progress in the successive development of the peculiarities which characterize them as natural divisions among animals. Such forms I shall call *Progressive Types.*

The relations, however, which are exemplified in the oldest Sauroid fishes, in the Ichthyosaurians, in the Pterodactyls or in the Megalosaurians, seem to me to be clearly of a different character, and to differ from simple progressive types, inasmuch as those which appear earlier, combine peculiarities which at a later period appear separately in distinct forms. For instance, the reptilian characters which we recognize in the Sauroid Fishes, are developed at a later period in

animals no longer belonging to the class of fishes, but constituting by themselves new types, provided with additional peculiarities which separate them fully from the fishes in general, as well as from those fishes in which we recognize some relation to reptiles during a period when no reptile existed.

Again the Ichthyosaurians, though true reptiles appearing long after fishes had been called into existence and during an early period of the history of the reptiles, still show their relation to fishes by the character of their vertebral column, and foreshadow, as it were, in their form, the Cetacea of later ages, as well as many forms of the gigantic Saurians of the secondary period. The same may be said of the Pterodactyls, which are also true reptiles, but, in which the anterior extremity foreshadow peculiarities characteristic of birds and bats. Such types I shall call *Prophetic Types.*

To an analytic mind the examination of the peculiarities of such animals may foretell a higher progress of development, carried out in real existence, only during a later period, even if he had never seen the later ones; for in such types the germs of a future development may be recognized, and upon close examination, truly referred to the peculiarities of other higher groups, even if the intermediate links remained unknown, which, however, as matter now stands, can leave no doubt in our mind that these prophetic types really foreshadowed that diversity of forms which has been created since they have gone by. We may also say that these prophetic types lay before us the course of thoughts which has been carried out in the plan of creation by the Supreme Intelligence who called them into existence in such order of succession and in so diversified relations. The recognition of this prophetic character of certain types of extinct animals, is not only important in a philosophical point of view; I have no doubt it will ultimately and rapidly lead to a better, fuller, higher and deeper understanding of the various relations which exist between animals. Let me at once point to some of these relations which might never have been understood but for this appreciation.

Among Crinoids, we have not only progressive types, as I have already quoted, but we have also prophetic ones. The Cystidæ are truly prophetic of the Echini proper. I may only mention the genus Echinocrinus to show the link.

The Pentremites again, are the prophetic type foreshadowing the starfishes. And often in subordinate groups we may find such close relations between genera of the same minor divisions; such, for

instance, as the genus Encrinus, in which the genera Apiocrinus and Pentacrinus are simultaneously foreshadowed. Perhaps, in this case, a distinction might be introduced between truly prophetic types, and synthetic types, in which the characters of later groups are rather more combined than really foreshadowed.

As for the relation between older types and the embryos of the living representatives of the same families which are so extensively observed in almost all groups of the animal kingdom, which have existed during earlier periods, it may best be expressed if we call those fossils which exemplify, in full grown animals, forms which exist at present only in the earliest stages of growth of our living animals, *Embryonic Types*, in counterdistinction from the progressive types and from the prophetic types. These embryonic types may be purely such, or they may be at the same time either progressive types, or even prophetic types. I shall call purely embryonic types those in which we recognize peculiarities characteristic of the embryo of the same family. For instance, the older Sauroids which have the upper lobe of the tail prolonged, or the common Crinoids provided with a stem, which resemble the young Comatulæ, &c., &c., I shall distinguish as progressive embryonic types, those in which we recognize simultaneously a relation to the embryo of the same family, when they form besides a link in a natural chain of progressive development. Such, for instance, as the oldest Salamanders or the earliest Sirenoid Pachyderms. Finally, I shall call prophetic embryonic types those in which we have embryonic characters combined with the peculiarities which stamp the type as a prophetic one, such, for instance, as the Echinoid and Asteroid Crinoids of the former ages.

The fact that these different types may thus present complications of their character, or appear more or less pure and typical, goes farther to show how deeply diversified the plan of creation is, and how many relations should be simultaneously understood before we are prepared to have a full insight into the plan of creation. There we see one type forming simply, and alone the first link of a progressive series. There we see another which foreshadows types which appear isolate afterwards. There we see a third which, in its full development, exemplifies a state which is transient only in higher representatives of the same family. And then again we see these different relations running into each other and reminding us that however difficult it may be for us to see at one glance all this diversity of relations, there is, notwithstanding, an Intelligence which not only conceived

these various combinations, but called them into real existence in a long succession of ages.

The following papers were presented by Prof. Agassiz, but from want of time the substance only was mentioned and no abstracts have been furnished for the proceedings.

On the classification of Gasteropoda with reference to the systematic position of Pteropoda and Foraminifera.

On new species of Crustacea, from the shores of Massachusetts.

On the difference between Sirenidæ and true Cetacea, and the embryonic characters of the former.

On the homologies of Acephala and Gasteropoda with reference to the systematic position of Brachiopa and Bryozoa.

On new species of Echinoderms, from the shores of Florida.

On new genera and species of Bryozoa, from the shores of Massachusetts.

On new genera and species of Annulata, from the shores of Massachusetts.

On the ovarian Egg of Birds and Allantoidian Reptiles.

On the histology of Invertebrated Animals.

On the homologies of Annulata and Caterpillars.

On the ovarian Egg and embryology of Aphides.

On several genera of Entomostraca recently established; which have been ascertained to be embryonic states of Decapoda.

On the embryonic character of Palæozoic Crustacea.

On the changes which fresh-water Turtles undergo during their growth.

On the embryology of Calyptræ.

Colpoda and Paramæcium are larvæ of Planariæ.

On the Composition of the Plant by Phytons, and some Applications of Phyllotaxis. By Prof. Gray.

Prof. Gray made some verbal statements, with illustrations, as follows:—

It was my intention to take this opportunity to develop some points in vegetable morphology, but as the hour is now so far advanced, I shall be very brief, intending these remarks chiefly to prepare the way for some interesting mathematical deductions, by my colleague, Prof. Peirce. The principles of morphology are now inwrought into the science. Every botanist knows that each plant consists, in

its whole development, of an axis throwing out from place to place organs identical in their origin, identical in the laws of their arrangement, and nearly identical in structure, but which are made to subserve different functions, according to their position on the axis. They are collectively called *leaves*, because their usual development is *foliaceous*, but they frequently assume other forms, and are subservient to other uses. They subserve that process in which vegetation essentially consists, namely, what is called vegetable digestion ; so that we may properly take the leaf as the typical form of the organ. Then for other purposes they are transformed into very different shapes, and as the organ is to subserve other functions, it assumes different forms, some of which, in outward appearance, are very unlike leaves. If we take the flower, for example, we find it to consist, first, of a true set of leaves, those of the *calyx*. That *petals* are of the same character, is evident, notwithstanding their brighter colors and more delicate texture. There is no other difference. That the other organs, the *stamens* and the *pistils*, are of the same character, is a deduction easily made, thoroughly supported by a long chain of evidence, and now admitted by all botanists. These floral organs stand on an axis, or short stem, (which is itself a prolongation of the leaf-bearing stem,) just as the leaves stand on the stem. The plan of the foliage is carried on through the flower. They are all parts of one and the same system. The laws of one apply also to the other. The leaves of the calyx and the petals of the corolla are leaves still in their form, the latter merely changing their color, as a general thing. The filament or stalk of the stamen answers to the stalk of the leaf; while the anther consists of two parallel lobes, each representing the portion of the blade of the leaf upon each side of the stalk, and made fleshy by a large development of cellular tissue ; and this develops not into parenchyma, but into pollen. The opening to shed the pollen occurs in a line which corresponds to the edges of the leaf.

In the pistil the nature of the transformation from the typical foliaceous form is different, but is easily to be made out. The pistil answers to a leaf infolded so that its margins join and unite, forming below the ovary or seed-vessel, which bears its ovules, or rudimentary seeds, on what corresponds to the margins of the component leaf; while a more or less prolonged and attenuated apex becomes the style, and some part which answers to the margins, destitute of epidermis, forms the stigma and conducting tissue.

Vegetable morphology has usually been studied in this way only. The flower, taken as a whole, with its several parts, has been cons-

pared with the leafy branch. But a closer examination shows that we must make a further analysis. The branch itself is an individual only in the sense in which the whole tree may be so termed — only as a branch of coral is so called.

The term *metamorphosis*, used by many botanists, is not well chosen; at least, no proper comparison is to be instituted between this and the metamorphosis of animals, where the simple, entire individual undergoes great changes in the course of its development. The diverse forms and functions which the leaf assumes are rather to be compared to the homologous organs of animals; that is, an organ fundamentally the same develops in one case as a leaf, in another as a petal, in another as a stamen or a pistil; just as the forearm, the false wing of a bat, the wing of a bird, the pectoral fin of a fish, &c., represent one and the same organ; only the same individual plant presents all these forms successively, in the different stages of its development from the cotyledons, or seed-leaves, up to the pistil; because, in fact, the plant is not, like the animal, a simple individual forming its parts simultaneously, but a compound individual, or rather a congeries of individuals, successively produced.

This brings us to the analysis to which I alluded. I do not mean merely that the branch is a new individual developed on the parent stem, and the branchlet, a third generation, produced by the branch. Every physiologist, every attentive observer, is led at once to this analysis. But the branch, or simple stem itself, is manifestly an assemblage of similar parts, placed one above another in a continuous series, developed one from another in successive generations. [Here the leafy branch of a poplar, and of a reed, was divided into its component elements or individuals by severing each internode at its base.] Each one of these joints of stem, bearing its leaf at the apex, is a plant-element; or, as we term it, a *phyton*, — a potential plant, having all the organs of vegetation, namely, stem, leaf, and in its downward development even a root, or its equivalent. This view of the composition of the plant, though by no means a new one, has not been duly appreciated. I deem it essential to a correct philosophical understanding of the plant.

The study of the development of the plant confirms this. The first phyton, in the simplest case, preëxists in the seed. We have it in the monocotyledonous embryo. (In the plumule, when manifest before germination, we have already the rudiments of one or more new phytons.) This has already its stem-part or radicle, and its leaf-part, the cotyledon. The name "*radicle*" is an unfortunate one, as

it has led botanists, almost without exception, up to the present day, to regard as a rudimentary root that which is properly the first joint of the stem. From its base a root is developed, indeed; but the radicle itself is no part of the root. It is the primitive internode of the stem, the initial portion of the ascending axis, and it develops according to the laws of the latter, and not at all after the manner of the root. In any germinating plantlet it is easy to see the point of junction of the descending axis, or root, with the base of the stem, the inferior extremity of the radicle, so-called. As the primitive phyton produces from the lower extremity its root, which, avoiding the light, buries itself in the soil, from the upper (which by the elongation of the axis is usually carried above the surface and exposed to the light,) the cotyledon expands into a leaf; and the plantlet is now perfect, being possessed of all the organs of vegetation. Now, or even before the first phyton is fully developed, it produces a second phyton from the apex of the stem-part of the first; this in turn gives rise to a third, and so on. It is the apex of the leaf which first appears, according to the usual law of growth of all exserted parts (just as the finger nails grow from the base;) soon its stalk, if it have any, is disengaged, and finally its stem-part, or internode, appears and elongates until it gets its whole growth. (Prof. Gray here illustrated the development of a monocotyledonous plant, from the seed through to the pistil, on the blackboard.) Thus the second and each succeeding phyton exhibits its leaf and stem; if we detach it as a cutting, we may cause it to produce the remaining organ by striking root in the soil, and thus acquire complete independence. Or, even while connected with the parent phyton, we may cause it to strike root by bending it down to the soil. Or sometimes they will spontaneously strike root in the open air, at some distance above the ground, as we may observe in the lower joints of vigorous stalks of Indian corn. This happens so frequently in tropical plants, as in the mangrove, banyan tree, &c., as to show that it is no casual or very extraordinary exception. That a portion of the wood in such cases which would otherwise contribute to the increase of the main stem, is thus diverted to the ground, placing the leaves of the branch or upper part of a stem in direct communication with the soil by their own roots; that the woody fibres of the leaf may be traced into the joint of the stem that bears it, and thence into the one below; that each annual layer of an exogenous tree is in direct relation with the onward growth of the branches of the year, making a new formation from the apex of

the branchlets down to the extremity of the roots, actually representing the downward growth of all the phytons of that year, blended together, and placing the leaves in communication with the distant rootlets in the soil; these are some of the numerous facts and considerations which there is no time left to illustrate, that convince us that each phyton makes a downward growth, which answers to the root produced by the initial one in germination, or to its woody system, at least; and completes the evidence that the vegetable is made up by the repetition of similar or homologous parts, each of which is a potential plantlet; — parts which usually develop separately above, while they are blended together into a common mass below. In other words, that the comparison of a tree with a mass of arborescent coral is a just comparison. The stem-part of each, which soon belongs in part to all the phytons above, is persistent. The leaf, or foliaceous portion performs its temporary offices, and falls away.

Besides the lineal succession of phytons, each one at a later period produces, or may produce, a second, and sometimes a third and fourth, in the axil of the leaf, that is, between the base of the leaf and the phyton above. These give rise to branches, or secondary axes.

The development of the dicotyledonous embryo is the same, only it is double that of the monocotyledonous, or consists of two phytons, with their stem-parts collaterally united. Its normal development therefore gives rise to opposite leaves. It may continue to bear leaves in pairs throughout, as in the maple; or one of the pair may be suppressed after the primordial leaves, as in the Locust, or immediately after the cotyledons, as in the Cherry. [Prof. Gray illustrated the development of the dicotyledonous embryo on the blackboard; and demonstrated the flower as composed of phytons with their stem-parts exceedingly abbreviated.]

The simplest perfect and complete flower, therefore, consists of a simple green foliaceous organ of one leaf (*calyx*;) of a second, usually of a delicate texture, a *petal*; of a third, which has lost the foliaceous appearance and bears pollen, the *stamen*; and then a fourth which bears ovules, the *pistil*. But it rarely happens that the flower is thus simple. Nature seldom trusts so much to one individual, but multiplies their number to ensure the result.

In Monocotyledons there is usually an assemblage of three, or some multiple of three; in Dicotyledons usually five or some multiple of five (rarely four,) of each sort.

However assembled, the floral organs, if our view be well founded,

should conform to the law which governs the arrangement of the leaves themselves. The normal plan in this respect, that is, the position which the successive phytons assume in respect to each other, I may very briefly illustrate, as it will serve to introduce Prof. Peirce's mathematical exposition of this subject, which is to follow. In the simplest mode, with only one leaf on each node, the leaves occupy two vertical ranks; the second leaf stands on the side of the stem exactly opposite the first, but higher up; the third is opposed to the second or immediately over the first; this occurs in all true grasses, and in many other plants. In the next, and equally common case, the leaves are three-ranked, having an angular divergence of one third of the circumference of the axis, so that the fourth stands over the first, the fifth over the second, the sixth over the third, and so on indefinitely; that is, the phytons are developed in a spiral order, fixing three leaves in one turn of the spiral round the circumference, and the fourth over the first, beginning the second turn, &c., as illustrated by a sedge-grass. In the next plan, the most common of all in dicotyledonous plants, five leaves are laid down in each cycle (the sixth being over the first,) two revolutions being made around the stem to accomplish this, the leaves therefore occupying five vertical ranks, the angular distance between the successive leaves being two fifths of the circumference, &c.

Dr. Gray proceeded to illustrate with specimens, the next case, where eight leaves are laid down in three revolutions; the next, with 13 leaves, in five revolutions, and so on; and to show how the expression of these different modes in the form of the fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{13}$, (in the manner now familiar to botanists, and which is illustrated in recent elementary works on the subject) develops at once the connection between the several modes, the numerators of the two antecedent fractions added together giving the numerator, and the sum of the two denominators giving the denominator of the next in the series. And the series farther extended in this way to $\frac{8}{21}$ $\frac{13}{34}$, &c., is verified by observation.

The same arrangement should occur in the flower. It is natural to consider the calyx as one complete cycle, and the corolla as the next, and so on. This rule, therefore, would place the leaves of the corolla directly over the leaves of the calyx, &c., while it is a well known fact that they are over the interval, or alternate with them. The petals occupy the place left between the leaves of the calyx. The floral organs regularly alternate with each other, and this creates a puzzling anomaly.

Let us take a flower of five parts, with five leaves to the calyx, five petals, &c. We should suppose it to be upon the two fifths arrangement; but this would bring each petal exactly over a leaf of the calyx, instead of between them. Botanists have been much puzzled to explain this, to find any satisfactory mode of bringing the habitual position of the floral organs into accordance with that of the leaves, so that both might be expressed by the same law. When this is done, the proof of the doctrine that the leaves and floral organs are homologous parts will be complete. Prof. Braun, who, with Dr. Schimper, first developed these laws of phyllotaxis, and who has worked them out very elaborately in all their manifold applications, by introducing the term *prosenthesis* to express the singular deviation, or pushing forward by half an interval, which appears to take place in passing from each floral verticil to the next, has only stated the exception, without explaining it. The only attempt at explanation that I am aware of, which is at all satisfactory, has recently been made by M. Adrien de Jussieu. Instead of assuming that the trimerous flower is actually formed on the plan 1-3, and the pentamerous flower on the plan 2-5, he projects them both upon the 5-13 arrangement, and shows that this gives the floral organs in threes or in fives, so nearly in their position of regular alternation, that, with the tendency of all growing parts to develop on the side of least pressure, they could hardly fail to fall into that position. It is well known that in irregular flowers, organs are often suppressed or rendered abortive on the side where most pressed, and are pushed out of place towards the side of least resistance.

[Prof. Gray illustrated this view by a diagram, and showed upon the proposed plan, how little displacement was required to bring about the actual alternation.]

Mathematical Investigation of the Fractions which occur in Phyllotaxis. By Prof. B. Peirce.

The Association may wonder what a mathematician can have to do with Botany, and what right he has to discuss such a subject as vegetable morphology. But let me assure you that the geometer is somewhat omnivorous in intellect, and although he has lived and thriven for centuries upon the sun and moon, the planets and comets, and other such inorganic food, he is already aspiring to a vegetable diet,

and may ere long be whetting his teeth for flesh and blood. But, in the present case, the botanists have provoked the invasion by undertaking to demonstrate that plants grow according to exact mathematical laws. They have presumed to measure with minute accuracy, and exact measurement must open the path to geometry. They have dared to use our numbers and fractions, and we must reclaim them with interest.

Upon the principal points, which have occurred to me in the investigation of the curious fractions of vegetable morphology, I apprehend that I have been anticipated by Bravais. But it may be that I have developed the subject more distinctly than he has done, and am therefore disposed to present my views to the Association. I do not regard these fractions as isolated and independent of each other; but all of them seem to me to be approximations, more or less accurate, to one and the same fraction, or rather to several fractions *of one series*. It is as if in the forms of vegetable growth, there had been one great thought underlying the whole structure. The thought has in it an element of infinity, but the mode of expression is necessarily finite. It is everywhere partially developed, with more or less approach to perfection in different plants. This very defect of expression has enabled us to discern and comprehend the divine idea with our finite capacities. Had it been fully expressed, we should not probably have discovered it. Most certain is it that if the infinite fraction had been introduced into the creation, we could not have detected it; for the infinite series would not have been completed, even though the tree had grown to heaven itself.

The fraction cannot be fully written out, but the law of its formation is easily perceived. It is $\frac{1}{n + a}$ in which n is any positive integer, while a has a peculiar value. The quantity a is an infinite continued fraction, formed wholly of units; thus, —

$$a = \cfrac{1}{1 + \cfrac{1}{1 + \cfrac{1}{1 + \cfrac{1}{1 + \text{\&c., ad infinitum.}}}}}$$

The successive approximations to the value of a, are obtained in the way familiar to mathematicians by stopping at the different points of the downward train of units. They correspond identically to the fractions which have been observed in the finite forms of the vegetable universe. They are, 1, $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{5}{8}$, $\frac{8}{13}$, $\frac{13}{21}$, $\frac{21}{34}$, &c.

The corresponding values of the fraction itself of vegetable growth are, when n is 2,

$$\tfrac{1}{3},\ \tfrac{2}{5},\ \tfrac{3}{8},\ \tfrac{5}{13},\ \tfrac{8}{21},\ \tfrac{13}{34},\ \tfrac{21}{55},\ \tfrac{34}{89},\ \&c.$$

which is the series usually observed. The values, when n is 3, are

$$\tfrac{1}{4},\ \tfrac{2}{7},\ \tfrac{3}{11},\ \tfrac{5}{18},\ \tfrac{8}{29},\ \tfrac{13}{47},\ \&c.,$$

which are also found in nature, but much more rarely than the preceding row of fractions. These, if I do not misunderstand the botanists, are the only fractions which directly occur in the ordinary forms of growth. The few other fractions, which have been observed, are the products of indirect species of development which are rarely exhibited; but even these fractions are included in the general form. They correspond to the cases in which n is 4, 5, or 6.

This expression of the forms of vegetable growth in a fraction, leads to the inquiry why this particular fraction was selected. It obviously gives an admirable packing to the leaves, as Dr. Gray has shown, and this may justly be regarded as a sufficient *a priori* reason for its adoption. It must be admitted, however, that other excellent systems of packing might be devised, which would appear to be very little inferior to this; the fraction $\tfrac{7}{18}$ would give a good arrangement, and the same might be said of an infinity of other fractions. The exclusive adoption of one and the same series of fractions in all vegetable forms, must therefore be sought, partly in the desire to construct a system sufficiently simple and uniform to admit of human investigation and study, and at the same time so complicated and varied as to command his wonder and admiration, and partly in the uniformity of the laws of vegetable growth through the machinery of which it must be introduced and developed. It cannot be supposed that the leaves attain their proper position through a voluntary selection of the widest spaces. They all start to grow at once, and each phyton occupies at the outset the very place to which it belongs. It seems to me most probable that beneath this series of fractions lies the fundamental law of organic action, and the simplicity of the series confirms this opinion.

I must now take the liberty to draw the attention of the Association to another domain of the physical universe, in which there are distinct traces of these same fractions. They are approximate expressions of the relative times of rotation of the successive planets of the solar system. Thus the ratio of the mean motion of each planet to that of the next inner planet is nearly equal to some one of these fractions. This is so manifest, that all the great inequalities of long

period which occur in the solar system depend upon these ratios, and they are interwoven with all the most important irregularities of motion of the primary planets. Whence could this extraordinary coincidence have arisen but from the action of a single mind? and what does it indicate but that the same Word which created the planet, is expressed in the plant?

May I close with the remark, that the object of geometry in all its measuring and computing, is to ascertain with exactness the plan of the great Geometer, to penetrate the veil of material forms, and disclose the thoughts which lie beneath them? When our researches are successful, and when a generous and heaven-eyed inspiration has elevated us above humanity, and raised us triumphantly into the very presence, as it were, of the divine intellect, how instantly and entirely are human pride and vanity repressed, and by a single glance at the glories of the infinite mind, we are humbled to the very dust.

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